THURSDAY, MAY 15, 1884

DANIELL'S "PHYSICS"

Text-Book of the Principles of Physics. By Alfred Daniell, M.A., Lecturer on Physics in the School of Medicine, Edinburgh. (London: Macmillan and Co., 1884.)

AN important and welcome addition to the existing text-books of elementary physics has appeared. Since the days when Arnott's and Golding-Bird's manuals were in vogue, the number of text-books of physics which have appeared in this country is very small as compared with the vast number of excellent manuals for students which have been produced in Germany and France. Many of these are unique. Verdet's "Cours de Physique" stands alone; nothing like it has ever been produced in England. The text-book of Jamin and Bouty, now much enlarged, is also unique, and of semi-mathematical text-books is by far the best, though limited by the curious restriction that seems to cramp all French science,—ignorance of all scientific work that is done outside France. Jamin's smaller *petit traité*, though admirable, is too concise. Daguin's text-book is overloaded: those of Fernet, and of Boutan and D'Almeida unequal in balance, and too obviously cut to suit the narrow requirements of the *baccalauréat*. The same remark applies less strongly to Ganot's excellent "Physique." Those of Moutier and Violle are yet incomplete. In Germany, Müller-Pouillet's "Lehrbuch," recently overhauled and enlarged by Prof. Pfaundler of Innsprück, is a grand and substantial work for students. If it lacks the elegance of Jamin and Bouty, it makes up in solidity and catholicity of information. Its wealth of pictorial illustration is unapproached. Wüllner's "Lehrbuch" is heavy, and his "Compendium" is not what students want. Mousson's "Lehrbuch," now greatly enlarged, is valuable as a work of reference, but might with benefit be pruned. In addition to these we might name hosts of others by Viktor von Lang, Jochmann, von Waltenhofen, Eisenlohr, Koppe, Emmsman, R. Waeber, Hessler-Pisko, Paul Reis, Krebs, Crüger, Sumpf, and others. Against this array what can be shown in Great Britain? The English translation of Ganot by Dr. Atkinson has long held sway; its indefatigable editor has long ago filled up the gaps of the original French work; but it has grown almost encyclopædic, and has never quite freed itself from Ganot's academically conservative way of treating physical problems. The lesser elementary Ganot is also excellent in its way,—as a purely introductory book. Besides the translated Ganot we have also the translated Deschanel; a work which, thanks to Prof. Everett, is vastly superior to the original French work, and has proved of great value as a text-book, by reason of the excellent cuts and the valuable mathematical editorial notes. Dr. Everett's lesser "Physics" is also good as a purely introductory work. In addition to these we may mention, though adapted for popular instruction rather than for students' reading, the two volumes of Guillemin on the "Forces of Nature," edited by Mr. J. Norman Lockyer, which have done good work in their way. So far, not a single really English text-book! But, stop; there is one genuinely British and of very original merit, Prof. Balfour Stewart's "Elementary Lessons in Physics." This comparatively small volume may be cited as the first conscientious attempt to rewrite "Physics" from the modern standpoint, namely, on the basis of the doctrine of energy. In this respect it is infinitely ahead of the more ambitious adaptations from French authors, and will probably long keep its place as a text-book for elementary work. We do not forget Galbraith and Haughton's manuals, excellent—and sketchy—as they are; nor the re-edited Lardner volumes, which, in spite of the abilities of Messrs. Carey Foster and Lœwy, are very decidedly of the *réchauffée* order, and should be allowed an honourable burial. There are also C. Bird's handy "Notes," and a volume by Dr. Aveling, of which, the less said the better. This strange poverty in modern text-books would be indeed remarkable were it not that it is more than compensated by the great abundance of splendid text-books on isolated branches of physics which have issued from the press of Great Britain during the past decade. Individual treatises on mechanics, optics, electricity, sound, and heat have to a considerable extent supplanted more general treatises on physics, with great advantage, in the long run, to the solidity of the reader's information. Nevertheless the text-book of physics has its place and its readers. Men who are reading for mathematics, for medicine, or for the army still require in many cases a something more than superficial acquaintance with physics. It is they who buy the Ganots, the Deschanels, and the Balfour Stewarts, and find them more or less adapted to their needs.

Mr. Daniell's new "Text-book of Physics" is addressed to such students as these, and more particularly to those who read physics as a part of their preparation for a medical degree. But there is abundant matter in it for the engineer and the chemist to consider. The work opens with two chapters on the fundamental notions of mass, time, and space, and on the derived kinematical and kinetical quantities. The third chapter deals with measurements, and the fourth with work and energy. All this is only introductory, and occupies but 50 pages. The remaining 570 pages are therefore based upon the doctrine of energy. The chapter on Kinematics which follows is lengthy but admirable. Harmonic motion, and problems connected with the propagation of waves, are expounded with great care. The teaching here, as throughout, bears the impress of the two great living Scotch physicists whom Mr. Daniell names as his masters. The chapter on Kinetics is none the less welcome because the author frankly gives up the misleading term, "centrifugal force." It strikes us that more might have been said with advantage concerning angular motion and the problems relating to couples. A very useful chapter on Attraction and Potential is then introduced: a most sensible step, since too often the notion of a potential function is postponed until the student comes upon it quite unprepared in electrostatics. It may be noticed that a similar course is adopted in the text-book of Jamin and Bouty. Gravitation and the pendulum conclude the kinetic section of the work, which next deals with the properties of matter. In this chapter, which must be extremely interesting to those

who approach the subject freshly, modern ideas come to the front. The question of viscosity of solids and liquids comes prominently forward. The views of Crookes on the "radiant" condition of highly attenuated gases are discussed, and so is also the connection between the liquid and gaseous states. With respect to the alleged continuity between these states, the author does not seem to have quite made up his mind; for though he quotes with approval the proof given by Ramsay, that the so-called critical point is not necessarily a phenomenon of continuity at all, but simply a certain condition of things in which the liquid and its vapour mix, because they have arrived at equal density and cannot keep separate, he seems also to lean to the other view, apparently relying on an unconfirmed observation by Hannay on the solubility of a solid in a gas. The vortex theory of atoms and the existence of the ether are mentioned in this chapter. There is a capital section on elasticity, and another on liquids, in which the molecular phenomena of liquid cohesion and surface-tension are given due weight, as are also sundry matters concerning the kinetics of liquids, often omitted from such text-books. A short chapter on Gases is introduced, and then follows one on Heat. Just fifty-one pages are given to this entire subject; but in those fifty-one pages an enormous amount of useful matter is comprised. There is not a superfluous line or even word. This extraordinary success—for the thing is most successfully done—is largely due to the author's fundamental method of starting from the energy doctrine. The chapter on Heat opens with the first law of thermodynamics, and states it thus: "Heat, being a form of energy, can be measured in ergs." The rest of the subject is developed in a masterly way; though probably the student who has read nothing previously in this branch will find it tough. The paragraph on "the six thermal capacities" is very suggestive, and needs clear thinking to follow it. The author adheres, not quite wisely, we think, to the practice of taking as the definition of the calorie or heat-unit, the kilogramme-water-degree unit instead of the gramme-water-degree unit. This is the only case in which the author does not accept the C.G.S. system. Is there any adequate reason why he should not follow the more modern custom and adopt the *calorie mineure* instead of the *calorie majeure?* The chapter on Sound is also well written; and, for the first time, so far as we are aware, we have the notation of the tonic sol-fa system introduced along with staff notation into the discussions of pitch and temperament.

The chapter which succeeds is enough to take away one's breath. Were this a text-book of the stereotyped academic style, one would know exactly what to expect. After Sound, Light: a chat on the velocity of light; the old familiar gray-headed problems of reflection and refraction; a glance at the rainbow and at telescopes and microscopes; and, to wind up, a couple of pages on the spectrum—with the inevitable chromolithographed chart—and two more on polarisation. But this is not the method of our author. He heads his chapter, "Ether-Waves," and after a little preliminary clearing away he launches into radiation and introduces notions on wave-length, heating effects, colour, and on *exchange* of radiations. Prevost's law and Stokes' law lead direct to the analysis of radiations in the spectrum, and to the evidence afforded in the spectrum of the phenomena of transmission, reflection, and absorption. The propagation of waves through the ether next comes up, involving the questions of plane and circular polarisation, and then, after all this,—shades of the immortal Potter and of the revered Todhunter!—come reflection and refraction of light, mirrors, prisms, and lenses! This is indeed a *bouleversement* of the time-honoured custom of giving all attention to geometrical optics, leaving physical optics to take its chance at the fag end. Yet we are persuaded that the method is essentially right. It is to be regretted, however, that the author does not, with all his improvements, adopt Gauss's treatment of lens problems. Perhaps this is solely for want of space; the sin, if it be one, is one of omission only. Separate sections on interference, double refraction, optical instruments, and rotatory polarisation are given. The instruments are briefly but satisfactorily discussed; the ophthalmoscope and stereoscope receiving due attention. The last and longest chapter in the book is devoted to Electricity and Magnetism. This chapter, though abounding in good points, is to our mind the least successful of the whole; it will not satisfy electricians, though it may, and will, give to medical students a very good and thorough insight into the phenomena and laws of electricity. A very useful bibliography of works on physics for further reading closes the book.

One further point strikes us in reviewing the book as a whole—the excellence of the examples chosen to illustrate the problems and remarks. Particularly to medical students will this feature recommend itself. Levers and moments of forces are illustrated at p. 151 by a long list of articulations in the human skeleton. The references on p. 154 to the action of the biceps and deltoid muscles, on p. 138 to Rosapelly's researches, on p. 143 to anæmic disorders, on pp. 252-55 to the relations of the physical processes of osmose and diffusion to the tissues of the body in relation to juices, foods, alkaloids, and to serpent-poisons, will be recognised as giving a distinctive character to the work.

Such criticisms as it remains for us to pass are directed solely to a few points in which the author will do well to modify the work when it shall attain—as it doubtless will—to a second issue. In the section on the rainbow the secondary and tertiary, &c., bows due to multiple internal reflection are apparently confused with the supernumerary bows due to interference. On p. 35 the "watt" is wrongly defined as a unit of work, and equal to 10^7 ergs, whereas it is not a unit of "work" at all, but a unit of "activity," and is equal to 10^7 ergs *per second.* The matter recurs on p. 575, but is not there much mended. The author uses the letters "E.M.D.P.," meaning thereby "electromotive difference of potential," for that which is more commonly denominated "electromotive force," and abbreviated into "E.M.F." We cannot think the change well advised. What would the author do if he came to discuss the formulæ of a dynamo, in which the induced electromotive force is a very different quantity from the difference of potential between the terminals? Would he write both "E.M.D.P."? Again, on p. 625, the author uses "utility" for the quantity commonly called the "efficiency" of an electric motor. This is in itself not perhaps a bad exchange of terms. But the author goes

on to misapply the very same word and to use it in the sense of "activity," saying in effect that (by Jacobi's law) the "utility" is a maximum when the reaction of the motor reduces the supply-current to one-half. The diagram intended to represent Hall's experiment on p. 628 is quite wrong. The statement on p. 503 that the velocity of light is greater in metals than in air is incorrect: the refractive index is greater, therefore the velocity less. The author has most wisely abandoned the use of that most misleading of terms, vapour-tension, and substitutes therefor simply pressure. This is well; but the reform must go further, and should have gone further at the hands of so worthy a pupil of the Scottish school of physical precision. A reference to the index shows apparently that the author uses the word "*pressure*" correctly and consistently in the sense of force per unit area. If this were so, it would be excellent. Unfortunately the text of the treatise is not always consistent. On p. 152 the author talks of applying a "pressure" to a lever, where he does not mean so many dynes per square centimetre, but where he means simply a "force,"—a push. Of course this confusion of language is pardonable: it runs riot throughout every Cambridge text-book of mechanics from Todhunter to Garnett. We had hoped better things here. Again on p. 154 comes the following question:—"A nutcracker 6 inches long has a nut in it an inch from the hinge: the hand exerts a pressure of 4 lbs.: what is the stress on the hinge?" Answer: "The stress on the hinge is the weight of 24 lbs." In the first place the word "stress" is wholly misapplied; for a stress is not a force, but a force divided by the area on which it is applied: and in the second the word "pressure" is equally misapplied, because what is meant is that the hand applies a "force" equal to the weight of four pounds. In like manner the author's general precision of language would lead to the expectation that he would not misapply that unhappy word "tension." Referring to the index it appears that he uses the word tension in four different senses. He speaks of "surface-tension" of a liquid: which is excusable if the words are connected by an indissoluble hyphen. He speaks of voltaic cells being coupled in "tension," where he means united in series. He speaks (p. 522) of atmospheric pressure in an electrified soap-bubble being resisted by "an electric self-repulsion or *tension* over the surface" (as if self-repulsion were a pulling instead of a pushing force!); and lastly, he speaks of the "tension" of a string (p. 142) when he means the pull, not the stress of so many dynes per square centimetre. He is no worse, however, than the majority of writers on the subject. The average Cambridge text-book teems with similar instances, where problem after problem is set to "find the tension in a rope," without the necessary data as to area of cross-section in the rope being given. There is one book on physics, now happily almost extinct, in which the word tension is used in eight different significations!

Lastly, we must congratulate Mr. Daniell on having embodied the latest results of contemporary research in his work. The ordinary text-book is not seldom ten or fifteen years behind in its data, in some cases more. Here, however, we find, absorbed into the fibre of the book, the most recent matter, such as the researches of Lord Rayleigh on the unit of resistance, of Quincke and of Worthington on capillary phenomena, of Guébhard on electro-chemical figures, of Crookes on radiation and "radiant matter," of O. E. Meyer on viscosity, of A. M. Mayer on the analysis of sounds, of Rosenthal on animal heat, of Vierordt and of Chauveau on blood-pressure, of Wintrich on the use of resonators in auscultation, and of Abney and of Langley on dark radiation. A text-book so furnished forth is doubly welcome. S. P. T.

RECENT CHEMISTRY

Experimental Proofs of Chemical Theory for Beginners. By Prof. Ramsay. (London: Macmillan, 1884.)

The Discovery of the Periodic Law, and on Relations among the Atomic Weights. By John A. R. Newlands. (London: Spon, 1884.)

Chemical Analysis as applied to the Examination of Pharmaceutical Chemicals. By Messrs. Hoffmann and Power. Third Edition. (London: Churchill, 1884.)

Chemical Analysis, for Schools and Science Classes. By A. H. Scott-White. (London: Laurie, 1884.)

Facts Around Us. By C. Lloyd Morgan. (London: Stanford, 1884.)

Science of Food. A Text-book specially adapted for those who are preparing for the Government Examinations in Domestic Economy. By L. M. C. (London: George Bell and Sons, 1884.)

An Outline of Qualitative Analysis for Beginners. By J. T. Stoddard. (Massachusetts, 1883.)

THERE is not the least doubt that in English laboratories theory does not occupy a prominent position. Prof. Ramsay is to be complimented on this very small book, which is certainly a valuable attempt to put chemical theory on a practical basis. It consists of a series of exercises on the measurement of temperature, pressure, and weight in connection with gases, &c., and contains very valuable directions and instructions in the manner of reading thermometers and barometers, and representing changes by curves, and also in the graduation of thermometers and other instruments in common use in the chemical laboratory, a work which should not be relegated entirely to a physical laboratory. The work is divided into twelve chapters, in all of which we have very excellent practical exercises on what is commonly known as chemical theories, that is, the sensity of gases, the law of Gay Lussac and Avogadro, on quantivalence, specific heat, and the equivalents of metals, and a short chapter summarising Newlands' work on the periodic law of the elements. We can strongly commend this little book to all students in chemical laboratories.

Mr. Newland's little book, as the author says in his preface, contains an exact reprint of all the papers on relations among the atomic weights and on the periodic law written by himself and printed in the *Chemical News* during the last twenty years. In its present form it is a desirable addition to our literature, and should bring Mr. Newlands' very valuable work into its proper position.

Messrs. Hoffmann and Power's very elegant work is evidently not adapted to an ordinary chemical laboratory. As the authors state, it has been prepared for the pharmacist and dispensing practitioner of medicine, for the purpose of enabling him to test chemicals and drugs used in pharmacy. The arrangement of the work is

therefore such as to suit the practical pharmacist rather than the analytical chemist. The substances are described under their several Latin synonyms, in addition to the French, German, English, and Spanish names. We find that each preparation is described as to physical and chemical properties, and then follows a very elaborate examination for the presence of impurities, in addition to methods of quantitative determination of the principal constituents. It should be a very useful addition to the pharmaceutical laboratory.

Mr. Scott-White's volume is one of the usual little books of chemical analysis tables. There seems to be nothing very remarkable about it, excepting the variety of types in which the formulæ are printed. The book, which is intended as a text-book for the various examinations of the University of London, Oxford and Cambridge Senior Locals, and the Kensington examinations, seems well adapted for its purpose. It contains a table of solubilities of common inorganic salts, which is a thing students rarely make use of; and an appendix of requirements in examinations, detailing apparatus, chemicals, &c., necessary for most of the elementary examinations in chemistry.

With all our science classes and the very general spread of scientific education throughout the country, it is still a sad fact that the great mass of the public and even of the middle-class educated public are wofully ignorant on common things. Even now it is somewhat out of place to talk in a drawing-room about oxygen: as to the mention of phosphorus or selenium, or metals like platinum or iridium, it is still more out of place. A great deal of this ignorance—ignorance possibly occasioned by dread—is doubtless caused by the very scientific science books that are in common use. We are almost entirely without books on general science that are sufficiently simple, and at the same time accurate, to convey a general but correct notion of ordinary substances, or to interest the ordinary reader in all these things around us. Why should not the properties of oxygen or phosphorus be quite as interesting reading as some of the three-volume novels? Mr. Lloyd Morgan in his very small book has evidently intended to supply to some extent this want by describing—not in simplest language, it might have been simpler—a few very common chemical and physical facts. It does not appear exactly from the preface for what class of readers it is intended, but it can scarcely fail to be useful if not interesting to any lay readers. It commences with the chemistry of a candle flame, and in that way passes on to the similar actions taking place in animals and plants, where of course carbonic acid comes into play, and we are led through carbonic acid to wood, coal, and diamonds, to the atmosphere, where the physical part comes in, the pressure of the atmosphere, the thermometer, and the idea of elements, compounds, and mixtures. Passing on to water, we have the proof of the composition of water, physical properties of water, which leads directly on to the phenomena of heat. Although only consisting of about 150 small pages, we are led up at the end to some chemical reactions, and an appendix on molecules. The whole book is arranged for experimental purposes, although the methods of performing some of the experiments are not given. It has been probably assumed that the experimenter should have conveniences supplied. The appendix on arithmetical questions seems scarcely required in such a work, but, excepting this, it is certainly a step in the right direction to bring a knowledge of common things into a simple and understandable shape.

"L. M. C.'s" text-book is a sort of chemical, physiological, and biological book, and is divided under the following heads:—Food, its composition and nutritive value; its functions; and its preparation and treatment. It is evidently got up for the purpose of preparing for the examinations, as it says in the introduction that a grant of 4*s*. will be given for a pass, and that payment upon the results of examinations of school children are made to managers. In spite of this a considerable amount of useful information may be obtained from it, although that information is not conveyed in the best possible style. The descriptions of substances like bread, for instance, are not by any means exhaustive. Under animal food or flesh, it is stated that "animal food is composed of the same materials as vegetable; it is formed of the same elements and presents the same approximate principles, and contains water and mineral matters of the same kind as plants." This is not very instructive. The section on food and its selection will be useful, but the main object of the book is evidently to prepare for the examinations on this subject.

If the spread of chemical teaching may be measured by the number of small books on qualitative analysis, it certainly has a great number of disciples. There is nothing very extraordinary in Mr. Stoddard's book, unless it be the importance that is given to the atomicity marks attached to the signs of the elements. We find that iron is described as Fe^{ii} and also Fe^{iiii}, whilst chromium is only put down as Cr^{iiii}. Nickel and cobalt are both marked as dyad and tetrad. Only the so-called ordinary elements and acids are treated of.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]

Reply to Mr. Grubb's Criticisms on the Equatorial Coudé of the Paris Observatory

In continuation of my first letter I now wish, in my turn, to criticise Mr. Grubb's instrument, and to show that in all respects it bristles with inconveniences. I discuss it as it is presented and explained by Mr. Grubb, and I wish to examine it successively—

(1) From the optical point of view.
(2) From the point of view of the mechanism employed.
(3) From the point of view of its application to astronomy.

(1) The optical point of view. The system adopted by Mr. Grubb is much inferior to that generally used in ordinary equatorials. The dialytic telescope only gives images free from colour for the point which lies in the axis of the object-glass. For all other points images present themselves under the form of spectra which are longer as they are further from the axis. This arrangement necessitates that the three lenses must be very exactly centred, which can be done with the ordinary achromatic object-glass where the two lenses are in contact in the same cell. It is with very great difficulty that this can be done in a dialytic telescope. While admitting, however, that it can have an optical axis common for all the lenses of which it is composed, this centering becomes very difficult when the images are broken by a plane mirror, the

angle of which varies, and it becomes almost impossible in a broken equatorial, in which the eyepiece is independent of the moving part of the instrument, as proposed by Mr. Grubb. In a word, taking no account of the new and very grave causes of variability introduced by Mr. Grubb, this optical system is so unstable that its employment has been rejected unanimously by all astronomers and opticians. The least derangement of the position in the central mirror spoils everything.

The mobility of the plane mirror presents equally, from the optical point of view, a slight inconvenience. The quantity of light varies with the different angles of inclination, which renders the exact researches of the photometric very difficult. Without making one feel all its gravity, Mr. Grubb has, in truth, indicated the defect of this optical system. But in order to turn the difficulty he suggests that, since the field of view becomes smaller as the instruments become larger, we may content ourselves with observing at a central point. But this is an affirmation pure and simple. It is necessary in many measures of precision to have a large field of view. The contrary will present several serious objections. We have, in fact, to observe stars in relation with other stars, to measure, for instance, the difference of declination between a planet and a star of comparison. But we cannot make both these observations at the centre. The same thing will always be happening, in the case of comets, nebula, and clusters. It seems to me, on the contrary, that a telescope is more perfect the larger the field of view. Feeling thus, I have had made by Prazmowski, for my new equatorial *coudé*, achromatic eyepieces giving a very large field. For the observation of comets I have such an eyepiece, which magnifies fifty times and has a field of view such that I can observe a degree. For a telescope of twenty-seven inches we might have such an eyepiece with a field of twenty-four minutes.

From all which precedes, I think everybody will agree that the system proposed by Mr. Grubb is far inferior to that now employed in ordinary equatorials.

(2) The mechanical point of view. The instrument consists actually of an ordinary equatorial, in which the part which carries the eyepiece is replaced by a counterpoise. It presents therefore, from the point of view of stability, all the defects of the ordinary instrument. Additional causes of instability inherent to the design are—

The micrometer and eyepiece are completely independent of the principal mass, which necessarily gives rise to different defects of decentering between the separated parts. Moreover, in this instrument there are three distinct movements. In addition to declination and right ascension, there is a third, which consists of a differential movement round the axis of the mirror. This last one constitutes alone, from the point of view of stability, a complication which does not exist in the ordinary instruments. All the movements of transmission are broken at a right angle, and are four times as long as those of an equatorial *coudé* of the same size. There will therefore be such loss in transmission, one would never have the instrument perfectly adjusted and oriented. We see, from the mechanical point of view, there is such an incoherence between the different parts of the apparatus, that it is inferior to those now employed. And it really cannot be compared with mine, which is almost as stable as a transit instrument, and in which the movements of transmission are excessively simple.

(3) If we examine Mr. Grubb's instrument from an astronomical point of view, we see it is based on a principle which no astronomer can admit, namely, that it is superfluous to observe the greater part of the northern heavens. In many investigations among the most elevated in the astronomy of precision —let us take stellar parallax as an example—one is obliged to combine observations made at different epochs of the year, and it is only by the combination of measures thus obtained that the desired result is arrived at. These stars must be observed, therefore, in the northern part of the heavens as well as in the others, for the vicissitudes of climate do not permit the astronomer to observe exactly how and when he wants. The same necessity presents itself in the study of the double stars; to ascertain and to discard the systematical errors in the angles of position the astronomer is obliged to observe these stars in all the celestial regions. If one wishes to limit one's self to the exploration of one side of the heavens, one would lose precious opportunities and gratuitously introduce serious difficulties. There are also many cases in which this choice is not possible. Thus, if we wish to discover new comets every part of the heavens must be explored, and if one wishes to observe them they must be observed where they are.

Finally, permit me to ask Mr. Grubb how he is going to study that part of the heavens which lies between 20° from the zenith and the Pole. This region of space, I take it, would be entirely closed to the observer with Mr. Grubb's arrangement. Any research, therefore, which touched the stars covering this large area could not be undertaken.

The independence of the micrometer of the rest of the instrument renders impossible any measures of precision. The orientation of the micrometer, in fact, is the fundamental base of every measure, and to do this preliminary work properly three or four successive operations have to be performed, and take the mean of the readings and adjust the apparatus by means of the circle of position. But this fundamental operation cannot be performed on Mr. Grubb's instrument. In fact, in practice, if one wished to take an angular measurement with this instrument, one would have to proceed somewhat in this wise: First of all it would be necessary to content one's self with one approximation as to the orientation; then to repeat this after every individual measure; and lastly to take into account the disorientation of the micrometer, to submit the readings of the circle of position obtained to fastidious computations with a view to compensate them. This gives an idea of all the inextricable complications in which one would find one's self involved in this case. In fact, to secure a simple observation of a comet it would be necessary to increase the readings and the calculation by four times, and after all one would only get a result inferior to that furnished by an ordinary equatorial. I don't believe there is a single astronomer in the wide world who would undertake observations of precision under such conditions.

It is quite true, as Mr. Grubb indicates, that the *oculaire* might be connected with the rest of the instrument, but then, new inconveniences of another order would arise. These, however, I will not discuss now, for, as I said at the beginning, Mr. Grubb's actual proposal is now alone in question. However this may be, I consider the conception of this equatorial is so defective, taken as a whole, that I do not think its adoption would be seriously recommended. Nor do I think that the project will go beyond its present stage, unless essential modifications are introduced, and in this case the instrument would become like my own.

Paris Observatory M. LŒWY

Dust-Free Spaces

I VENTURE to call attention to some points in connection with the observations on "dustless spaces," &c., as detailed in the report of Dr. Lodge's lecture published in NATURE, vol. xxix. p. 610.

Certain observations and studies of my own lead me to think that, if attention be given to the points to which I wish to call the notice of physicists, results of the highest importance may be reached by means of the method of experimenting developed by Dr. Lodge and Mr. Clark, and described in the report referred to.

Dr. Lodge's statement (p. 611) that "cloud spherules are falling, but falling very slowly," is true when these spherules are not at a higher temperature than the atmosphere in their neighbourhood. When, however, very small particles floating in the air become heated, they warm the air immediately surrounding them, and then these particles are either buoyed up by a small envelope of heated and dilated air clinging to their surfaces, or they are borne aloft by the local currents which they create by contact with the surrounding atmosphere.

Observations continued for nearly fourteen years have convinced me that in ordinary clouds these two methods of lifting are combined—that to a certain extent each of the spherules or very many of the spherules of clouds are buoyed by adherent heated and dilated air, and that the whole of the cloud, in many cases at least, becomes warmer than its neighbourhood in general, which adds to its buoyancy as a mass of intermingled air, water, and vapour.

These remarks apply also to small particles of matter other than water. The action is the same except in degree. The very high specific heat of water enables it to heat surrounding air more readily and quickly than other substances do, and as a consequence masses of water as in clouds are lifted more quickly and to a greater height than masses of other bodies having the same proportion of surface to weight.

If it be remembered that radiant heat passes uninterruptedly through air, *i.e.* that air is diathermous, it will be seen that radiations from a distance striking upon particles of athermanous bodies suspended in the air will cause these latter to heat the

air about them, and produce upward currents or a buoying of the athermanous particles by dilation of the air in contact with them.

This affords a complete explanation of cloud-flotation and the flotation of fine dust-particles. For some years I have been in the habit of watching clouds, and by the use of the above theory have very often been able to account for forms, dimensions, and movements which I could not otherwise explain. Some fours ago I explained the above ideas to the Chief Meteorological Officer of the United States Signal Service, and received from him suggestions which have since afforded me the means of much pleasure in observing the locations and forms and movements of clouds, and although these irregular masses are subject to many complicating circumstances, I have never yet observed anything tending to weaken this theory of flotation, but have made many hundreds of observations tending to confirm it.

I trust that it will be taken for granted that I do not wish to attack the hypothesis of Dr. Lodge and Mr. Clark, that heated bodies "bombard" and drive away approaching particles. My object is simply to show that, as it seems to me, the theory of particles buoyed up by a locally heated fluid, when considered in connection with well-known principles of radiation, &c., is sufficient to account for the phenomenon of the "dust-free coat" described in the article alluded to.

Referring to the figures on p. 612, an ascending current is shown in the neighbourhood of the pipe or rod in Fig. 1. The theory which I have sketched would indicate that this current had been set up in great measure by the *indirect* action of the heated tube or rod upon the surrounding air.

I should contend that the dust-free coat may be explained as follows:—

A given particle which may be assumed to be directly below the rod is heated by radiation from the rod. It in turn heats and expands the air in contact with it; the particle with a coat of adherent air becomes lighter than the surrounding atmosphere, and the mote, with its jacket of expanded air, ascends towards the rod. As it reaches the point marked "slow moving" in the figure, it begins to find itself in air which has been heated directly by contact with the rod, and distributed near it by the small "circular" currents which always surround a blunt obstacle in a stream of fluid. At the outer limit of the "dust-free coat" the particle or mote is arrested because it has come to a point where the air is so warm that the mote can no longer heat its jacket enough hotter than its surroundings to cause buoyancy. It is arrested because it has reached a point where the surrounding medium is as light as its own air-float, much as cork is arrested at a surface of water.

The mote with its warm air jacket could ascend through cool and therefore heavy air, but the air warmed by contact with the pipe is too light to float it.

The dark "tail" above the rod, or tube, is the upstreaming dust-free air, warmed by the tube, and too light to carry motes, or in which motes have not been carried by any current.

The report of the lecture contains within itself some very striking confirmations of this theory. For example, Dr. Lodge tells us that at a high temperature the dust-free coat is thicker than at low ones. This is according to the theory of flotation as above set forth, because an approaching mote would sooner meet the increased body of air warmed by contact with the tube to a point sufficient to destroy the buoyancy of the mote and its jacket. Again, hydrogen is a light gas having a very high specific heat; hence according to this theory the mote would need more heat and more difference of temperature to float than in air, and consequently should not be able to float up to as near the rod. Now, Dr. Lodge states that "in hydrogen it [the dust-free coat] is thicker than in air." With a surrounding medium of carbonic acid, less heat and less flotation are required for the mote, as the gas is heavier and of lower specific heat, and, quite in accord with the theory, the dust-free coat "is thinner" than in air or hydrogen. Again, Dr. Lodge states that the dust-free coat is set up by a "difference of a degree or two," and it would apparently require a much more complicated theory than the simple one here advanced to account for this on the bombardment hypothesis, as the action has been shown to be—

(1) Affected by the medium as to thickness of coat.
(2) Obtainable at different temperatures in the rod.
(3) Apparently dependent, not on the actual temperature of the rod, but on the differences in temperature between the rod and its surrounding dust-containing fluid.

The behaviour of cool rods or plates, as stated, is also in accord with this theory. A mote coming within the influence of the plate or rod is cooled by radiation and loses buoyancy in its air jacket. If above the plate, it therefore falls upon it; if below, it drops away. Dr. Lodge does not explain how a cool plate "bombards" the motes and drives them away from its lower side. If clearly explained, the method of experiment developed and now under study by Dr. Lodge and Mr. Clark, and that of Mr. Aitken on the condensation of water about nuclei, will probably be found productive of results of the very highest importance.

Questions of climate, rainfall, healthfulness of districts, fogs, mists, humidity, &c., can probably be better studied than in any other way by some form of apparatus based upon results obtained by these experiments, if the theory of flotation above set forth is connected with them, as I trust it may be.

EDW. W. SERRELL, Jun.

Chabeuil, Drôme, France, April 27

Mr. Serrell is no doubt perfectly correct in his view that the average specific gravity of a warmed and vapour-filled cloud may be often less than that of air. The ascent of the so-called "steam" from a kettle proves this, and he will find the view clearly stated in Maxwell's "Heat," p. 280. I did not enter into details in the Dublin lecture, but I was fully convinced of the truth of this statement.

His supposition that the dusty air near a hot body gets warmed not by gaseous conduction from the hot body but by interception of its radiation by the suspended particles, is not an unnatural one, but it is practically untrue. It is disproved by the fact that the concentrated radiation from the electric light is much less effective in warming dusty (or any other) air, than is the neighbourhood of a warm solid only a few degrees above the atmospheric temperature.

Mr. Serrell's criticism, that we do not clearly explain the down-streaming dark plane from a cool body observed by Lord Rayleigh, is quite legitimate. So far as I entered into the matter at all, I intended to indicate provisionally a distinction between a cool body and a very cold one—the boundary coming somewhere, say, between ten and thirty degrees below the air, or possibly depending upon actual temperature as well as on difference. I am not prepared to assert that the bombardment of particles towards a cool body begins the instant it is colder than the atmosphere. I think it possible that there may be a neutral point below which it begins.

But Mr. Clark is working out this among many other points, and I am not sure that his view at present agrees with my hypothesis. He will doubtless make a complete statement when he publishes an account of the quantitative research he is now engaged in. Till then I prefer to leave the account of cold bodies a little vague.

O. J. LODGE

The Supposed Volcanic Dust Phenomena

THE reddish circle round the sun, which I suppose must be considered as a kind of very large corona, alluded to by E. Divers of Tokio (NATURE, vol. xxix. p. 283), G. F. Burder (p. 525), and other observers, was invariably visible here, when circumstances favoured, from November 1883 up to April 3. In the middle of that day, and of the 4th, though circumstances seemed favourable for seeing it, no tinge of red was perceptible; but it was visible late in the afternoon of the 4th. Since then it has become more visible again, and from April 21 has been very plain, though not so conspicuous as it was originally. It is red in the middle of the day, and brown towards sunset, the bright space between it and the sun being blue or greenish.

The semicircle opposite the sun is now far fainter than it was originally, indeed I do not think I should notice it now without looking for it. It is now plainest when the sun is a little above the horizon, which was not formerly the case, and I have not seen it after sunset lately. This may perhaps be owing to a change in the height of the volcanic dust, or whatever it is.

The amount of sediment in the rain strikes me as being very large. I have at different times in the last few months collected it upon glass and examined it with the microscope: there appear in it a considerable variety of crystals and other transparent objects. Some of the crystals are like those drawn by Mr. Beyerinck (vol. xxix. p. 309). I have usually found a number of irregular transparent pieces, but I cannot say that they have

much resemblance to the vitreous ashes of Krakatoa, drawn on p. 587, as they are very thin.

I have examined this sediment with the naked eye to see whether I could perceive anything like the large corona. I darkened the room and admitted the sunlight through a narrow slit on to the glass. The sediment sparkles with various colours, chiefly pink and green, I suppose owing to interference; and it is difficult to judge which colour preponderates. I find a decided excess of green at a small angular distance from the sun, and often pink preponderates at a greater but varying distance. These colours being similar to those seen in the large corona are slightly confirmatory of the theory that the sediment from the rain is the substance which has caused it and the strange sunsets and sunrises; but other substances are also capable of giving a green light near the sun. Moisture on glass gives quite different colours, so far as I have observed.

The cirrus-like wisps on which the sunset phenomena appeared were definite and very small at the end of November; but on the whole grew larger and more indefinite, till at length they have been quite imperceptible for several weeks past.

On April 24 there was the first moderately bright aurora I have seen since October 5. Can this remarkable absence of auroras and the scarcely less remarkable frequency of lightning have been caused by the volcanic dust? If so, it may also account for S. Tromholt's finding auroras so scarce and poor in Iceland during the winter, as mentioned on p. 537 (vol. xxix.), though he does not say whether they were scarcer than usual there. THOS. WM. BACKHOUSE

Sunderland, May 10

Pons' Comet

PONS' comet was visible here with the naked eye throughout the month of February, including the nights of greatest moonlight. I so saw it on some twenty or more nights during that month, and append some notes as to its comparative brightness, so far as I could judge.

February 3.—"Comet visible till 10.45. Could see 'old moon' with naked eye easily, and in telescope Grimaldi and Aristarchus, but only with a very small part of sunlighted portion in field."

February 6.—"At 8.45 could see comet with naked eye, though sky not quite free of sunset-glow and somewhat hazy, and moon nine days old. It was altogether faint, but most of the tail visible at other times could be seen—certainly more than I should have expected."

February 9.—"At 9.30 found the comet with naked eye and could see it without difficulty, but there was only the suggestion of a tail. Comparing it with α Sculptoris by looking midway between the two, they produced the same effect on the eye; but of course the least magnifying power showed the difference."

February 10.—"8.20 to 8.50. Found comet with naked eye, but it was very faint, and to the unaided eye looked certainly fainter than α Sculptoris. Yet it seemed to me that more of the tail (or the tail more certainly) was visible than last night."

February 11.—"Found comet with naked eye about 8.10, and watched it up to 9.40. As the sky lost the traces of sunset I could pick it up without difficulty, in spite of the full moon shining in a cloudless sky. It was not quite so easily seen as α Sculptoris, but I may say that λ^1 and λ^2 Sculptoris, though each marked as of the same magnitude as α, I could not get a glimpse of, though I tried hard."

February 12.—"At 8.15 found comet with naked eye without difficulty, and so at intervals up to 9. Found it again with difficulty at 10.15; it was then getting low and into the haze: in the telescope it seemed then to have lost (at a guess) half its light."

On the subsequent clear nights in February there was no difficulty.

March has been much cloudier, and owing to this and moonlight I only saw it with naked eye certainly on four nights—the 1st, 4th, 16th, and 17th. My note for the 4th is: "Found and saw comet with naked eye several times, though not easily, between 8.15 and 8.40 p.m. Could see the outline of the 'old moon' without difficulty."

The 5th is marked as doubtful both as to comet and "old moon."

March 14.—"A fine pink glow in evening, and splendid afterglow about 7.15—never saw it better. Found comet easily with opera-glass, but could not see it with naked eye, the moon rising before the glow had vanished." So also on the 15th.

March 16.—"Saw comet repeatedly with naked eye (looking a little above it) between 7.45 and 8.30. Sky very good."

March 17, 7.40 to 8.10.—"Found comet with naked eye, and saw it many times, looking a little above it; could not be quite sure of seeing it direct."

Owing to clouds I have only seen it on two nights since, the 24th and 28th; and that only with opera-glass and telescope.

Nelson, N.Z., March 29 A. S. ATKINSON

Snow and Ice Flora

IN the account of Prof. Veit Brecher Wittrock's interesting work on the Arctic snow and ice flora (NATURE, vol. xxviii. p. 304) your reviewer enumerates the countries and mountain ranges where red snow has been observed, but does not mention the Southern Alps of New Zealand, where as far back as 1861 this plant was observed by me. The fact that green and red ice have been found in these high northern latitudes, and that the unusual coloration has been traced to microscopic organic life is of special interest to me, as I repeatedly observed green as well as red ice amongst the glaciers of New Zealand, first at the head of the Rangitata River, as far back as February, 1861. At the time I published an account of this occurrence, which was reprinted by others (amongst others see Hochstetter's "Neu Seeland," 1863, p. 342). Since then during my alpine explorations I have repeatedly observed the same phenomenon, so that evidently at the Antipodes there occurs a counterpart of the Arctic snow and ice flora. It is to be hoped that some able botanist will some day do the same work for us that Baron Nordenskjöld and his able coadjutors have done for Greenland and Spitzbergen. JULIUS VON HAAST

Christchurch, N.Z., December 31, 1883

The Rotation Period of Mars

NOTWITHSTANDING his comparatively small diameter and slow axial motion, the planet Mars affords special facilities for the exact determination of the rotation period. Indeed no other planet appears to be so favourably circumstanced in this respect, for the chief markings on Mars have been perceptible with the same definiteness of outline and characteristics of form through many succeeding generations, whereas the features such as we discern on the other planets are either temporary atmospheric phenomena or rendered so indistinct by unfavourable conditions as to defy lengthened observation. Moreover it may be taken for granted that the features of Mars are permanent objects on the actual surface of the planet, whereas the markings displayed by our telescopes on some of the other planetary members of our system are mere effects of atmospheric changes which, though visible for several years and showing well-defined periods of rotation, cannot be accepted as affording the true periods. The behaviour of the red spot on Jupiter may closely intimate the actual motion of the sphere of that planet, but markings of such variable unstable character can hardly exhibit an exact conformity of motion with the surface upon which they are seen to be projected. With respect to Mars the case is entirely different. No substantial changes in the most conspicuous features have been detected since they were first confronted with telescopic power, and we do not anticipate that in future ages there will be any material difference in their general configurations. The same markings which were indistinctly revealed to the eyes of Fontana and Huyghens in 1636 and 1659, will continue to be displayed to the astronomers of succeeding generations, though with greater fulness and perspicuity owing to improved means. True there may possibly be variations in progress as regards some of the minor features, for it has been suggested that the visibility of certain spots have varied in a manner which cannot be satisfactorily accounted for on ordinary grounds. These may possibly be due to atmospheric effects on the planet itself, but in many cases the alleged variations have doubtless been more imaginary than real. The changes in our own climate are so rapid and striking, and occasion such abnormal appearances in celestial objects that we are frequently led to infer actual changes where none have taken place; in fact, observers cannot be too careful to consider the origin of such differences and to look nearer home for some of the discordances which may have become apparent in their results.

The rotation period of Mars has been already given with so much precision that it may seem superfluous to rediscuss the point, but it is very advisable to see whether recent observations

confirm the values derived from former results. The "Hourglass" or "Kaiser Sea," which is admittedly the most prominent mark on the planet, is a very suitable one for comparisons to find the intervals of rotation. Early in 1869 I saw it with a 4¼-inch refractor as it passed the central part of the disk. On February 2, 1869, it was central at 10h., on February 4 at 11h., and on February 5 at 11h. 30m.

I observed the same object in February of the present year with a 10-inch reflector (power 252), and noted it crossing the planet's central region at the following times:—

1884		h.	m.
February 14	... ...	5	55
15	... ...	6	35
19	... ...	9	5
22	... ...	11	4

I have combined my observation of February 4, 1869, with that of February 14, 1884 (as I regard this pair as the best obtained), to ascertain the rotation period. The interval includes 5487d. 18h. 55m. = 474,144,900 seconds. Correcting this for the difference in longitude between Mars and the earth at the two epochs and for defect of illumination (there is no necessity to apply any correction for equation of light, as the apparent diameter of the planet on the dates selected for comparison was about 16", and hence the distances were nearly the same), I find the time of rotation resulting from the discussion of these observations to be

h. m. s.
24 37 22·34 (5349 rotations),

which is in satisfactory agreement with the periods computed by Kaiser, Schmidt, and Proctor from a much longer series of observations. In order to exhibit the small differences between the period now computed and those resulting from some of the best modern determinations, I give the following summary:—

	h.	m.	s.	
J. H. Mädler ...	24	37	23·8	*Ast. Nach.* 349.
1864, F. Kaiser... ...	24	37	22·62	*Ast. Nach.* 1468.
1866, R. Wolf	24	37	22·9	*Ast. Nach.* 1623.
1869, R. A. Proctor ...	24	37	22·735	*Mon. Not.* vol. xxix. p. 232.
1873, F. Kaiser	24	37	22·591	*Annalen der Leidener Sternwarte*, vol. iii. p. 80.
1873, J. F. J. Schmidt	24	37	22·57	*Ast. Nach.* 1965.
1884, W. F. Denning	24	37	22·34	

It is obvious that Mädler's period of 24h. 37m. 23·8s. is about one second too great. If we take a mean of the other six values (all within 0·6s. of each other) we get

h. m. s.
24 37 22·626

which may be fairly regarded as a very near approximation to the true sidereal rotation period of Mars.

The computations of Kaiser, Schmidt, and Proctor are severally based on very long periods, the comparisons being modern observations with those of either Huyghens or Hooke during the last half of the seventeenth century. It is unfortunate, however, that there is some question as to the correct identification of the spots depicted in some of the ancient drawings. The representations by Hooke on March 2, 1666 (old style), at 12h. 20m. and 12h. 30m., also those by Huyghens in 1659, 1672, and 1683 give a large irregular spot, extending in a north and south direction, which can only be identified as the "Hourglass" or "Kaiser Sea." It would appear, however, that this interpretation is incorrect in certain cases, for the several drawings do not only show disagreements with each other but also when compared with modern observations originate discordances of period, small it is true, but still too large to be attributed to simple errors of observation. No doubt the period which approaches nearest to the truth will become apparent from future observations, though it can hardly admit of definite settlement for many years, inasmuch as the differences between the several times of rotation as above deduced are very insignificant, and must so closely accord with the real period of the planet that the errors such as exist must be allowed to accumulate over a lengthened interval before they will become distinctly manifested. A comparison extending over fifteen years is insufficient for the purpose, for a computed time of rotation, erroneous to the extent of one-tenth of a second, will still, at the termination of such a period, answer to the positions of the markings to within 9 minutes of time. It is to be remarked that Mr. Marth, whose opinion is entitled to great weight, has, for some time, adopted the period of 24h. 37m. 22·626s. for the rotation of Mars. This corresponds to a daily rate of 350°·8922, and forms the basis of his computations in his "Ephemerides for Physical Observations of Mars," annually published in the *Monthly Notices*.

W. F. DENNING

"The Electrical Resistance of the Human Body"

WILL you kindly publish the inclosed from Prof. Dolbear? It furnishes a complete explanation of the discrepancy between his measurements of the resistance of the human body and those which I have recently made. At the same time, as I have pointed out to him, the fact that this resistance may sink below 500 ohms with "soaked skin," even if that be "abnormal," is of the highest physiological importance, and goes far to explain the hitherto mysterious deaths from accidental passage of a current through the body. Most of these, as Prof. Forbes remarked to me, have taken place with alternate, not continuous, current machines.

W. H. STONE

Wandsworth, May 11

College Hill, Mass., April 23, 1884

DEAR SIR,—I have to acknowledge the receipt of your pamphlet "On the Resistance of the Human Body," for which I am obliged. I am glad to know that physiology has some one in its ranks who is interested in that line of work, and who knows what to do in order to settle such vexed questions.

I have also seen in the last *Electrical Review* that has reached me an article on the same matter, in which you refer to me and what has been published concerning some of my work, that needs a little elucidation. In the early days of telephony the experiment was often tried of making the human body part of the circuit in order to see how speech could be transmitted through the body, in the language of those days. Bell wanted to know what the resistance of the body was when in such circumstances, and I measured it from hand to hand when thumbs and fingers grasped the terminals of a wire and found it to vary between 6000 and 15,000 ohms, and wrote to him to that effect, and from that grew out the statement to which you have referred. Now *under such conditions* that work is right, as I have frequently since proved.

It seems to me that when we speak of the resistance of the body or of any body, and do not define what is meant by body, it is fair to assume that the body is the ordinary body under ordinary conditions. If the resistance (the *actual*) of the wire is found to be a thousand ohms by one party and another one files off the rust from the contacts and then finds the resistance less, both parties may be right. Now the skin of individuals is more or less horny in texture, and so has high resistance which soaking may reduce, and the question then properly arises, is the hard skin properly a part of the body? The resistance of a farmer's hand is often twice as great as that of a child's or of a man of sedentary habits, but solely, as I think, because of the thickness and density of his skin. Does not the question resolve itself into this—What is the resistance of a dry hand and the resistance of a soaked hand? What is the resistance of a good conductor and the resistance of a poor conductor? If the poor one is made better in any way, its resistance is correspondingly increased.

If the condition of the body is abnormal, its resistance may also be abnormal. I should call a soaked skin abnormal.

Still it is of the utmost importance that we should know what the resistance is under all conditions, as being the only way to advance in knowledge of the physiological effects of known currents, and I would again express my gratification at your persistent work in this field, and if I can in any way be of service to you I shall be pleased to be employed.

Yours very truly,

To Dr. W. H. Stone A. E. DOLBEAR

Instinct in Birds

MR. GRAVES, who writes on this subject (NATURE, vol. xxix. p. 596), is, I fear, not so accurate an observer as the magpie, for he misquotes the day fixed by the birds for building, and then indicates that the young "mags" are restricted to four in each nest, while the fact is there are often six or seven in a nest. The magpie is too fond of a fresh egg for breakfast to escape the attention of the gamekeeper. I have often seen the greater part of their nest shot down, repaired, and reoccupied by the birds year after year. I know of no bird that begins the work of *nidification* here early in February, nor any that devotes two months to the work. The rook (*Corvus frugilegus*) is the first to

begin, and I have often been told that it does so early on the first Sunday of March, G.M.T.

What I said about the magpie beginning on the first Sunday (old style) was founded partly on report, but mainly on personal observation extending over some years at one breeding-place, where I have often seen them at work for the first time on this particular morning, and on one occasion in another locality on the same day.

This instinct is not confined to any particular tribe or order, but is common, I think, to all wild fowl, and the two instances given by Dr. Rae (vol. xxx. p. 7) of the regularity with which certain birds pass north to their breeding-grounds is precisely the point at issue, as I believe they begin work as soon as they arrive.

Scientific accuracy has not yet been directed to the subject, but there can be no doubt that from some cause, possibly a sharper and better defined division of summer and winter in former ages, all the feathered tribe have inherited an instinct in nest-building and in the time of their arrival at and departure from their breeding-grounds which guides them to a day in many cases without reference to the state of the weather.

WM. BROWN

112, West Regent Street, Glasgow, May 5

Watts's "Inorganic Chemistry"

THE review of my "Inorganic Chemistry" in NATURE of May 1 (p. 3) appears to have been written without much knowledge of the previous history of the work. The reviewer, indeed, writes as if he were criticising an entirely new book, whereas a glance at the preface might have shown him that the volume in question is the first part of the thirteenth edition of Fownes's well-known "Manual of Chemistry," the first edition of which was published in 1844.

H. WATTS

151, King Henry's Road, N.W.

The Recent Earthquake

I NOTICE that Mr. Topley, at the conclusion of his communication to you respecting the recent earthquake in Essex, remarks, "but at present we know of no observations in the central parts of Kent, Surrey, or Sussex." I wish therefore to mention, that although I did not myself notice anything in connection therewith, yet an invalid neighbour of mine, lying in bed, distinctly heard a rumbling noise about 9h. 20m., and a moment afterwards perceived some pot plants in front of his window sway to and fro. This is the only incident with which I have been made acquainted.

C. L. PRINCE

The Observatory, Crowborough, Sussex, May 3

THE rise in the Essex waters detailed in my letter of last week still continues. Mr. Radforde Sharpe has kindly sent me the following additional heights that the water rises from Messrs. Courtauld and Co.'s well, at Bocking, Braintree, in inches above the surface of the ground:—

May 6	$40\frac{1}{2}$ inches	May 9	$39\frac{1}{2}$ inches
" 7	$38\frac{1}{2}$ "	" 10	$39\frac{3}{4}$ "
" 8	40 "	" 12	44 "

At Colchester Corporation Waterworks Mr. C. Clegg, C.E., reports the rise recorded is still maintained.

Museum, Jermyn Street, S.W.

C. E. DE RANCE

W. H. FRANCE.—Any good entomological text-book will give you the information you ask for.

NOTES ON EARTHWORMS

EVER since our great naturalist called attention to the common earthworm, we watch them with entirely different eyes as they creep timidly out on to the lawn or hurry across the gravel walk; as they collect the dead leaves or bits of string and cloth we may have dropped the evening before, or heap up their household refuse outside the entrance to their home.

He long ago pointed out its importance as a geological agent. The surface of the ground would be very different were it not that the earthworm is for ever at work bringing in the decaying vegetation and converting it into mould. And, more than this, the superficial deposits are often modified to a considerable depth by the earthworms, which, carrying the earth mouthful by mouthful, and the gravel stone by stone, invert the order of stratification.

But we must not push this explanation of the origin of the universal surface mould too far. I received one caution from Darwin himself, many years ago when I was talking to him about the manner in which the chalk with which the land was dressed in Kent worked down. He told me to be careful to bear in mind the action of the great Kentish plough as it year by year turned swathe after swathe down the slopes. The result of this plough-down is clearly distinguishable from worm-mould. In his work on earthworms also he refers to another mould-forming agent of more universal operation and hardly less important cumulative effect. My attention was first directed to it by a lecture I heard delivered by Stoppani in Milan many years ago, in which he was explaining the action of the wind in modifying the surface of the earth, and especially in carrying dust, organic and inorganic. Richthofen and Drew have thus explained the origin of the loam that covers half Asia; and Mr. Clement Reid has recently extended the same kind of observation to Great Britain (*Geol. Mag.*, April 1884). Without this addition we can hardly explain how earthworms could find the material for the manufacture of the mould which often fills the interstices of the ruins of a buried city.

We find, commonly, isolated tumps of moss-covered soil, and every gradation from that up to the large patches of mould which hang like little gardens on each sheltered ledge, where the greater part of the material must evidently have been carried from elsewhere and not have been brought up from below; where it is obvious, from the character of the rocks, that the principal part of the mould cannot have been derived so much from them as from the wind-carried fragments of organic and inorganic material and the decomposition of the vegetation that soon began to grow upon it.

But we find also that the earthworms soon appear in such places, and set to work to mix up and modify all this various stuff that has by various agencies been brought together.

As squirrels, burying acorns and nuts in the autumn, have planted many an oak forest and hazel grove, so it is probable that the earthworms plant many of the ash and sycamore trees that we see perched in out-of-the-way corners, where it is difficult to explain how the blown seed can have got covered by mould enough to allow it to germinate. If an overhanging tree drops the seed, or the wind carries it anywhere near the worm's feeding-ground, it is dragged in and planted in leaf mould, and kept moist till spring time. At this time of the year we see clusters of sycamore seedlings growing up together out of the little worm-hills into which they had been dragged heavy end first.

It is therefore interesting to inquire into the various reasons that should make earthworms travel and occupy new ground. Round the margin of an overcrowded colony we should expect them to spread. They cannot live under water, so they have to move away before a flood. It has been stated that "they may live when completely submerged in water for nearly four months" (Romanes reviewing Darwin, NATURE, vol. xxiv. p. 553). But they were killed off by a flood of a couple of days' duration in the Backs of the Colleges at Cambridge in August 1879. Some of them seem to have got on to the paths, which are raised above the surrounding meadows, and there died. Where the greatest number were found dead the ground had been submerged for a longer time. The following carefully recorded observations by the Rev. Henry Russell, of St. John's College, are worth noting:—

"On Sunday, August 3, 1879, our paddock (the inclosed space in which the men play at lawn tennis, in front of the

new court) was covered with water to the depth, at 1 p.m., when it was greatest, of four to five feet. The level of the paddock is much lower than that of the ground surrounding it. . . . Therefore, on Wednesday, August 6, I cut a trench from the north-west angle of the paddock across the raised path. . . . The water had drained off by Saturday evening, August 9. The rush of water from the west across the Fellows' garden had carried with it into the paddock a great quantity of worms, which, when the water had subsided, were observed, some very large, lying dead under the water. As the water drained off, these lay on the paddock and on the slopes of grass surrounding it, and the smell of them infected the air till Friday, August 15."

Mr. Russell's observations go to show that the worms found dead were not all worms that had lived in the paddock, but those which had got washed out with the earth from the Fellows' gardens, and so they perhaps perished sooner being in the water. It is probable that worms buried deep in the earth under submerged meadows may, if they remain underground, hold out through much longer floods. However I gather that a large number perished in the adjoining parts of the Backs, and were seen on the paths and slopes as soon as the flood began to subside. Many of them were of exceptionally large size. I have heard of land injured by floods where the injury was supposed to be principally due to the destruction of all the earthworms. It is probable that the growth of peat-mosses may be in great part referred to the fact that the conditions were unfavourable to earthworms, for had they been there they would have worked up the vegetable matter into mould.

But there must be something besides floods that makes earthworms migrate.

If we drive a stick into the earth and move it about so as to shake the ground, the earthworms will come out to the surface and scuttle away in all directions. This was a common way of getting worms for fishing, and we used to be told, as Darwin notices, that the worms came out because they thought a mole was digging after them.

There must be however some other reason why worms will often come out to the surface in the daytime, and hurry away across a gravel path or on to a road, and why they then seem so much less sensitive to tremor of the ground about them than do the worms that come out to feed on the lawn.

From the analogy of other more highly organised animals I could not help thinking that there must be some creature that hunted the common earthworm, some worm ferret that drove them out. Many who have passed their lives in the country know well when they see a large field-mouse cantering down a road and showing little fear of man that a fiercer enemy than man is following the poor little animal with untiring certainty. If you draw aside and watch, you will soon see a weasel following by scent. Even a hare or rabbit will at length lie down paralysed with terror, and give itself up to the stoat that has followed it with deadly pertinacity. The sudden appearance of one or two strange birds in a neighbourhood has often been a source of wonderment, and it has sometimes been suggested in explanation that they had been chased by birds of prey and got up into strong currents of air. Those who have seen a peregrine drive a flight of rooks up into the sky can easily see how this might happen. In the cases to which I am referring the earthworm comes out like a hunted thing. I have also noticed that many of the worms that I found dead or torpid were maimed; generally they had their tail cut off, and this when there had been no digging in my garden for a long time, and although there are few birds that would touch them. I have frequently observed that the earthworms were apparently unwilling to go to ground again though I have tried to make them in order to watch the rate and manner in which they buried themselves. A few days ago, however, I saw, I believe, the explanation of most of the cases I had been observing. A large earthworm about nine inches long, bright, clean, and healthy-looking, was moving somewhat irregularly on the earth of a flower-bed. On stooping to examine it, I found a small yellow animal with a brown head holding on within about half an inch of the tail end of the worm. I sent it to Prof. Westwood, who writes: "Your worm-eating larva is evidently one of the Carabidæ, probably *Steropus madidus*" (see *Gardener's Chronicle*, 1854, p. 613). It was not disturbed by my taking up the worm, but went on biting its way round the worm, holding on like a bulldog, and bettering its hold every now and then. It had nearly got round the worm, leaving a lacerated ring. The wounded part seemed somewhat swollen, but on this point I am not clear, as the unequal power of extension of the wounded part may have produced the effect of swelling. Mr. Edwin Laurence has recorded (NATURE, vol. xxvi. p. 549) a similar circumstance observed by him in France, where, however, the larva seems to have attacked the worm differently, and with a view to killing it rather than cutting off a portion, and from his description, moreover, it would not appear to be the larva of the same species. He suggests that the numerous birds in England may have destroyed such an enemy of the earthworm. A sparrow would probably take the larva, and not touch the earthworm. One would have thought that the earthworm would have a better chance of rubbing off his deadly enemy in the earth than above ground, as a salmon is said to clean himself in a gravelly river, but we want further observations on this curious question, as well as on several others raised by the inquiry, How are worms transported to out-of-the-way places? and How long can they live in soils of various degrees of permeability when the surface is flooded?

T. MCKENNY HUGHES

THE LOW BAROMETER OF JANUARY 26, 1884

IN the end of January we gave a brief notice (see NATURE, vol. xxix. p. 316) of the unprecedentedly low barometric readings which were observed on the evening of January 26 in the middle districts of Scotland over which the centre of that great storm passed. The lowest reading, reduced to 32° and sea-level, then given was 27·332 inches, and was observed by Mr. George Croucher at Ochtertyre, near Crieff. This still remains the lowest reading observed during the storm, and as it is absolutely the lowest known to have been observed in Europe, if not indeed the lowest on any land surface of the globe since the invention of the barometer, it is desirable to give an accurate record of it in NATURE.

On that occasion, Mr. Croucher's observations included the barometer, its attached thermometer, and a thermometer hung outside the window, it being too stormy to venture out. The observations near the time of greatest depression, corrected for instrumental errors and reduced to 32° and sea-level, were, in inches, 27·631 at 7 p.m., 27·527 at 7.45 p.m., 27·420 at 8.30 p.m., 27·390 at 9 p.m., 27·332 at 9.45 p.m., and 27·365 at 10.15 p.m. The correctness of these readings is amply attested by the hourly barometric readings made at a considerable number of the Scottish meteorological stations that evening.

At the meeting of the Royal Meteorological Society on February 20, a paper was read on the storm of January 26, in which it is remarked that "the lowest readings of the barometer (reduced to 32° and sea-level) yet reported were 27·32 inches at Kilcreggan, and 27·332 inches at Ochtertyre." The observations at Kilcreggan were made with an aneroid, whose errors were unknown. From the hourly observations made at the different stations in Scotland, the isobars for each hour have been drawn, and, from a comparison of the Kilcreggan observations with these

isobars, the following approximate errors of the aneroid have been determined for the lowest recorded readings:—

p.m.	Aneroid, inches		inch
7	27·300	Approximate error	−0·230
8	27·200	...	−0·240
8.30	27·155	...	—
9	27·200	...	−0·230
10	27·300	...	−0·220
		Mean error ...	−0·230

If the correction + 0·230 inch for instrumental error and height be applied to 27·155 inches, the lowest observed sea-level reading at Kilcreggan was only 27·385 inches—a reading, it may be remarked, agreeing closely with the lowest readings noted at several stations on the mainland and islands of Argyllshire earlier in the evening. The Ochtertyre reading, 27·332, was thus, so far as known, absolutely the lowest recorded during the great storm of January 26, 1884.

THE THEORY OF SUNSPOTS[1]

THE literature of heliography, by no means inconsiderable in extent, has received an addition by the publication of the work before us which, if it makes no attempt to enlarge our knowledge of solar phenomena from personal observation, is deserving of notice as a specimen of one of the modes in which those phenomena are attempted to be explained.

The subject is confessedly full of difficulty as well as interest. Nothing can be more natural than the wish to obtain some knowledge of the constitution of that splendid orb that is the dispenser of life and enjoyment to unnumbered millions of organised beings, and that exhibits on its surface such a strange development of forces commensurate in intensity with its amazing magnitude. But these tempting inquiries are beset with difficulties scarcely to be appreciated in the absence of actual experience. When we bear in mind the amount of light and heat that has to be encountered, with all its consequences in optical, mechanical, and atmospheric impediments, we may rather wonder that man should have been permitted to accomplish so much, than that he should have failed in effecting more. The serviceable working of the telescope soon comes to an end; and what it is able to exhibit it is not able to render intelligible. In strong contrast with the exploration of the selenographer, who feels no doubt as to the general character of his object, whatever perplexities may arise out of the study of its details, the observer of the solar disk knows absolutely nothing as to what he is looking upon. He finds a blazing surface of by no means uniform texture, unlike anything else in the whole compass of his experience. He encounters strange-looking specks that disfigure, if we might venture to use such a word without presumption, the purity and perfection of that brilliant orb. In those dark patches, and their attendant fringe-like borders, what is it that meets the eye? Cavity? or cloud? or eruption? or cyclone? or scoria? Have astronomers succeeded in explaining them? Shall we listen to Wilson, or Herschel, or Kirchhoff, or Nasmyth, or Secchi, or Faye, or Zöllner, or Langley? More or less, they all disagree. Or shall we be venturous enough to attempt an independent solution of the mystery? Little encouragement could be found in such a course. After such protracted discussion we could hardly bring to our telescope an unbiased eye or an impartial judgment. What we are looking for, we should be likely to find. We shall be surrounded with phenomena that lend themselves with perplexing facility to very dissimilar and even opposite interpretations; and, where one observer is confident as to a clear vacancy leading down to unimaginable depths, another fills the same dark area with heavy clouds or floating dross. There may be, and for our own part we believe there are, as in the formerly contested theories of light, details of less equivocal character adequate to guide if not absolutely to establish our judgment; but the ambiguity of the general aspect is sufficiently shown by the support which such conflicting theories have claimed from it, each in its turn.

Perhaps we are disappointed in our telescope. It will be to no purpose to enlarge our aperture or deepen our eyepieces: we are still confronted by an insoluble mystery. We adopt a fresh mode of investigation, the means of which have been but recently placed in our hands; and we bid the spectroscope exert its analysing power and report to us what is there. And now, under the guidance of Lockyer and Janssen and Huggins, we shall be carried a long way in advance, further than the boldest imagination would have dared to anticipate but a few years back; and we find set before us, as in some strange vision, the unmistakable presence of familiar elements, ninety-three millions of miles away. Yet even this triumph of human ingenuity finds there a boundary that it cannot overpass. The evidence, to a great extent conclusive, is sometimes equivocal, sometimes perplexing: affected probably by influences the force and direction of which we can little estimate. The well-known features often wear a strange aspect, and are associated with incomprehensible surroundings. We have succeeded in interrogating the sun: he has answered us, and his answer will surely be reliable:—

> "Solem quis dicere falsum
> Audeat?"

That is, if we can but comprehend it; but unfortunately the message is not free from obscurity; some of it is in an unknown speech, and "Helium" and "No. 1474" and others of their companions are not only beyond our interpretation, but are likely so to remain. Very wide is the field thus opened for speculation, and very different may be the deductions from the same, or apparently the same, premises, with little possibility of demonstrating that any one combines all the elements of truth. Not one of the current theories has wanted defenders of intelligence and skill; if no one of them clears up all difficulties, no one fails in showing that there is much to be said in its favour; and therefore, as long as no patent absurdity interposes an insuperable bar, we may well exercise toleration to those who do not see through our eyes, or who question to some extent our conclusions. The best result is perhaps not very far in advance of probability, and every claimant has some right to be heard.

Remarks somewhat of this nature may be suggested by the treatise before us, which may be looked upon as an attempt to stem the prevailing current of opinion as to the cause of solar phenomena by showing that they may receive a complete explanation from Zöllner's theory of floating scoriæ, as expanded and developed by the author. The principal results which he has deduced from an extended collation, as it would appear, of the previous observations of others, may be expressed in the following way:—

The sun is to be looked upon as an intensely heated and very gradually cooling ball of monatomic gas, the visible surface of which, or photosphere, is, as Kirchhoff also maintained, composed of iron, with a small admixture of other metals, in a state of glowing fusion, and permeated in every direction by an abundance of incandescent hydrogen, this gas being poured forth abundantly from the exterior of the monatomic nucleus, where the central temperature is sufficiently reduced through decreasing density to admit of the first steps of elemental association. The presence and diffusion of this hydrogen maintains the fused condition of the iron shell, and prevents it from cooling enough to exhibit in every part the

[1] "Die Theorie den Sonnenflecken." Nach den neuesten wissenschaftlichen Forschungen dargestellt von J. E. Broszus. (Berlin, 1884.)

condition which obtains exceptionally in the spots. The "granulated" texture of the photosphere is the result of the eruptive pressure of the internal hydrogen, upheaving and penetrating the glowing mass of iron. The faculæ owe their greater elevation and intensity to an increased activity of the same process; and the chromosphere and the protuberances, whether of the more eruptive or more cloudy character, are traceable to the same origin, the greater brilliancy of the former class being due to the admixture of metallic vapours with the all-pervading hydrogen. The iron shell is not everywhere in a state of equal fluidity, a considerable portion being in a more "pappy" or viscous condition, such as may be seen in our own iron furnaces, which, however, does not render its presence manifest without such a reduction of temperature as to produce opacity. This cooling does not obtain either in the equatorial or polar regions, but is effected in what are known as the "spotted zones," by the overflow of hydrogen from the loftier equatorial strata of the atmosphere. Here, the gas, having been carried up in consequence of the solar rotation into a higher and cooler region, and extending itself laterally as an "equatorial current," descends on the less fluid portions of the photosphere, whence radiation is not so free; they are thus reduced to the more scoriaceous and opaque condition in which they assume the well-known appearance of "spots," while the whirls of cooler vapour on the outside of the main column in its downpour, encountering and tearing away the adjacent metallic edges of the chromosphere, force them to assume the form of those radiated fringes which we know as "penumbræ." The *maxima* and *minima* of the spots, as well as their respective drifting towards the poles or the equator, find their explanation in a "pulsation" or alternate compression and expansion of the globe, chiefly in the direction of its axis, from corresponding alternations in the balance of internal condensation and temperature, each of which is supposed to be in its turn in the ascendant; and though the change of dimension is slight, it is sufficient to give preponderance either to the equatorial or polar current, and, combined with the rotation, to determine the periodicity of the frequency and range of the spots. On the whole, the energy of solar radiation is never compensated; but the waste is so gradual that we have no reason to anticipate any sensible effect for ages to come, and yet so sure that the progressive cooling must terminate in ultimate extinction. In our author's words, "When in some future period of the world the whole of the hydrogen has escaped from the solar nucleus, the sun will cease to shine with its wonted intensity, and will become more and more feeble till at length it hangs in the firmament, a mighty globe of glowing red, as seen from other worlds a ruddy star, which, through rapid cooling, becomes visibly obscured, and, from the formation of everywhere surrounding scoriæ, immersed in deep night,"—a termination of which it may be said that, whatever its intrinsic probability, no reader need look forward to it with the slightest personal apprehension. And were that resplendent body, as Kepler in the exuberance of his imagination believed, the abode of glorious spirits, they might perhaps be supposed to smile at all such anticipations as utterly foreign to the unsearchable designs of the All-wise Creator.

And yet we may not forget that there have been, from time to time, mysterious warnings among the innumerable suns that have their abode in the far depths of space, and we are reminded by no process of argument, but by the evidence of our senses, how untrue it is that "all things continue as they were from the beginning of the creation." The certainty of strange and wonderful catastrophes of outburst or extinction has come to our knowledge, though perhaps only after centuries, or it may have been ages, of the transmission of the recording light: and similar events, to be recognised only by long-distant generations, may be in progress at the present hour. We know very little of the history of the universe, and it becomes us well to speak of such possibilities with caution and reverence. Meanwhile we owe a debt to all who will aid us in the attempt to gratify a very natural curiosity, and to our author among the rest. Some portion of his hypothesis does not come before us for the first time. La Hire in very early days entertained the notion of opaque bodies floating in a fluid mass and occasionally appearing on its surface; and the conclusions of Gautier were very similar as to a partial solidification of metal in fusion; but we must bear in mind that it is only for the diffusion of hydrogen through a liquid envelope of iron that our author claims originality. His ideas are expanded and enforced by so much elaborate reasoning as at any rate to deserve perusal, if they do not succeed in producing conviction. As to this point we may freely confess that the author is more sanguine than ourselves. Some of his arguments are well worthy of attention; but the general character of the treatise is that of an ingenious piece of special pleading, one-sided, but fair and honest in its self-persuasion. A few omissions and mistakes might be pointed out, but they do not impair his argument. The weakness of this, as our readers will have already perceived, lies in the magnitude of some of its assumptions. It might indeed be said that the same objection lies against each of the more commonly received theories; and to this it can only be replied that, though similar in character, it differs in amount; and that the value of any attempt at explanation must be estimated in the inverse ratio of its unproved demands upon our assent.

T. W. WEBB

THE EARTHQUAKE

IN a previous notice (p. 17) brief mention was made of the more obvious conclusions which follow from a consideration of the observed effects of the earthquake of April 22. Mention was also made of some points upon which further knowledge would be of value, notably as to the result of the earthquake upon wells and springs. Mr. De Rance's letters give important information upon this matter.

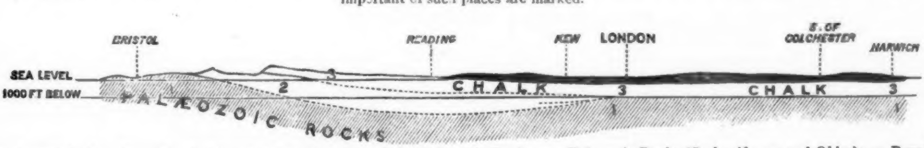
The measurements of the water in wells at Colchester and Bocking prove that the level of the water has risen seven feet in the former case and from twenty to thirty inches in the latter case. These facts, and also the curious instance of water spouting from the ground at East Mersea, are quite in accord with what frequently occurs during earthquakes. Mr. Mallet says:—"Fissures containing water often spout it up at the moment of shock. Wells, after the shock, alter their water-level, and sometimes the nature of their contents; springs become altered in the volume of water they deliver. . . . It is important to observe whether any changes of level of water in wells take place *prior* to earthquakes. Statements to this effect have frequently been made, but as yet stand much in need of confirmation."

Dr. Taylor's observations that the new and often slightly-built houses have generally suffered less than the old and more solid structures is scarcely what one would have expected. In districts much subject to earthquakes the houses are generally built in such a manner that they yield readily to the vibration, and so mostly escape serious damage. Mr. Mallet indeed believes that if this custom were enforced very little damage would be done. As regards larger and more important structures, the question is not so easily settled; and Messrs. D. and T. Stevenson, in constructing the lighthouses of Japan, employed a peculiar and ingenious contrivance for guarding against the effects of earthquake shocks: this was to interpose a break in the rigid part of the building, and so to prevent the propagation of the shock. Mr. D. Stevenson, in describing this, says:—"The plan I propose for this purpose, which may for brevity be termed an *aseismatic joint*, is

the introduction of spherical balls of bell-metal working in cups of the same material placed between two platforms, the lower cups being fixed to beams forming the foundation, and the upper cups being fixed to the lower beams of the superstructure, thus admitting, within a limited range, free motion of the upper over the lower part of the building." The cost of this was about 90*l.* for each lighthouse.

This plan had some disadvantages: high winds, for instance, gave as much free motion to the upper part as a slight earthquake would do; and there was also consider-

Map of the Earthquake of April 22, 1884. N B.—The places marked are those at which the shock was felt. In the East of England only the more important of such places are marked.



Section from Bristol to Harwich, showing the probable range of the Palæozoic Rocks. 1, Palæozoic Rocks (Carboniferous and Older); 2, Permian to Upper Greensand; 3, Chalk.

able movement of the lights during cleaning. The lighthouse-keepers, therefore, screwed up the metal plates, so that when an earthquake came sufficiently powerful to test the value of the plan, the structure was rigid and the glasses were broken. This plan was afterwards abandoned, and Mr. R. H. Brunton adopted the plan of constructing the lighthouses with "great weight and solidity, thereby adding to their inertia and checking the oscillation." Mr. W. Lloyd, from his experience in South America, believes that the more solid the structures the better they resist earthquake shocks. Mr. Woods, on the other hand, found that in Peru lightly-built structures of

iron were unharmed by earthquake shocks which did enormous damage to other buildings. This apparent contradiction may perhaps be explained by supposing that a lightly-built structure would give to every movement, and a very massively-built structure would resist such movement; either would bear a considerable earthquake shock unharmed, but buildings of intermediate resisting power would be destroyed.

Upon the twisting motion often noticed in earthquakes, and especially referred to by Dr. Taylor as observed at Langenhoe, Peldon, Fingrinhoe, and Abberton, much difference of opinion has existed. From the time of Aristotle it has been commonly attributed to a vorticose movement of the earth's surface at the part affected. The effects produced seem at first quite to accord with this explanation, as when in Japan a chimney ten feet high, and two feet by three, is broken in two, the upper half being twisted diagonally round without other fracture or displacement; or when, at Mendoza, a church tower has its lower story uninjured, its middle story turned through an angle of nearly 90°, and its highest story thrown completely over. But Mr. Mallet, who recorded similar cases in the Neapolitan earthquake of 1857, denied that this is in any case the correct explanation, and he believed that the twisting is best explained by resolved motions, due to the transit rectilinearly of the shock.

As regards the range of the recent earthquake very little more is known than was recorded during the first few days after the shock. The most important new fact is that mentioned by Dr. Prince of the shock being observed at Crowborough in Sussex; this proves that the Wealden area was affected, although the shock must have been very slight. Probably if the Palæozoic rocks were as near the surface there as was once hoped the shock would have been more distinctly felt.

There can now be little doubt that the origin of the shock was vertically under West Mersea or thereabouts, and that the wave must have travelled in all directions away from that area, but not necessarily with equal force and rapidity in all directions. The observations as to the direction of motion generally agree with this view; but in the neighbourhood of London there are some curious differences. The observations in London itself generally give an east and west direction, whereas some on the north side of London appear to point to a more north and south direction.

No observations are recorded of the connection of the earthquake wave with minor details of geological structure, such as the outcropping of certain hard or soft beds, or with lines of fault. The only instance of the latter kind known to me is at St. John's, near Greenwich, where the shock was felt very close to a fault, well exposed in the railway cutting just west of St. John's Station. But another and perhaps better explanation is that the shock was there felt by an invalid lying quietly in bed, and very sensitive to movement.

In the map here given, an attempt has been made to mark the positions of all places at which the shock was felt, so far as can be learnt from published accounts; but in Essex, Suffolk, and North Kent only a few of such places could be marked. By marking the outcrops of the older rocks (Carboniferous and earlier), the possible connection of these with the travel of the earthquake wave may be seen. This is made clearer by the section. The position of the Palæozoic rocks is known at Harwich and London; there is some uncertainty as to their position under Reading and Colchester, but for the purpose intended, and regard being had to the depth at which the shock must have originated (certainly far within the Palæozoic rocks), the line drawn is sufficiently near the truth. We can see how the shock can have been propagated through the hard Palæozoic rocks and been felt where these are bare or thinly covered with newer rocks, whereas through the thick and softer Secondary and Tertiary rocks the wave might travel a shorter distance. Possibly also this section may suggest an explanation of the double shock which was sometimes recorded: the first would be that travelling quickly through the hard Palæozoic rocks, the second that propagated more slowly through the softer overlying newer rocks.

W. TOPLEY

VOLCANOES ON THE SHORES OF LAKE NYASSA, AFRICA

DR. LAWS, on his return to Europe from the mission station at the north end of Lake Nyassa, passed by Naples, where I had the pleasure of meeting him. Amongst other information that I gleaned was that pumice-stone is very abundant in the locality above-mentioned and on the shores of the lake, where pebbles of coal are also met with. He also informed me that many of the rocks had a striking resemblance to the volcanic tufas around Naples. Dr. Laws happened to have a specimen of pumice, which he kindly placed at my disposal.

The specimen forms about two-thirds of a flattened ellipsoidal pebble of about $1\frac{1}{2} \times 1\frac{1}{4} \times \frac{3}{4}$ inches in diameter. It is of a dirty buff colour, darker in spots, the result of oil stains in packing. The grain is fine; there are very few large cavities, which are multilocular, with smooth-walled spheroidal-shaped alveoli. The specific gravity of the mass is light. The characters indicate great homogeneity of material, only a moderate amount of dissolved water in the original magma, and an eruption of true paroxysmal type. A few minute crystals of sanadin are discernible with the naked eye, and rarely also a small black spot, which we shall see to be pyroxene.

It is easily sectionised, and when examined under a low power, shows a remarkable uniformity of size in the pores. Those near and opening upon the surface contain a few diatoms indicating the action of water as the cause of the pebble-like form.

The magma is a perfect glass of light straw-colour. Scattered through it are a few small irregular crystals of sanadin, fairly clear, but of irregular boundaries in many cases, as if they had wavered between crystallisation and fusion. A few are twinned on the Carlsbad type, and a few also present fine wavy striation parallel to their longer axis. At one spot were two or three sanadin crystals inclosing dark brownish-green pleochroic microliths, too irregular to measure the angle of extinction, but which looked very much like amphibole. There were to be seen a few well-formed crystals of pyroxene of light pea-green colour, quite free from pleochroism, and with characteristic crystalline boundaries and cleavage, with absence of inclosures. In the immediate neighbourhood of the large pyroxene crystals were a few microliths of the same mineral; the average angle of extinction was 49°, and ranging within narrow limits. No other "formed" materials were discernible except a mass of dirty brown, dusty matter involved in a group of sanadin crystals, which might be magnetite. The whole character of the specimen is strikingly like some of the basic pumices of Monte Somma, and almost indistinguishable from some specimens of Phase VI. Period 1 (*Quart. Journ. Geol. Soc.*, January 1884).

I regret that for want of a balance I have not been able to analyse the specimen, though I am inclined to place it amongst volcanic rocks containing less than 55 per cent. of silica.

The specimen itself is in no way remarkable, but it is interesting as indicating the existence of continental volcanoes some hundreds of miles from the seashore, although in the immediate neighbourhood of a great lake, as also an additional grain of acquaintance with the geology of the mysterious interior of the "dark continent."

My informant has promised to forward me a collec-

tion of rocks on his return to Lake Nyassa, which will give us a more detailed knowledge of that interesting volcanic and also coal-bearing region.

Naples, April 26 H. J. JOHNSTON-LAVIS

NOTES

Two eminent chemists died on Monday, both born in the same year—1817—M. Karl Adolph Wurtz and Dr. R. Angus Smith, F.R.S. M. Wurtz, who was a pall-bearer at the funeral of Dumas, is stated to have died from the bursting of a blood-vessel; he was a candidate for the vacant post of Perpetual Secretary to the Academy of Sciences. Dr. Angus Smith had been in failing health for some time. A detailed account of the careers of both chemists we must postpone till next week.

THE Council of the Royal Geographical Society have selected the following travellers for honours:—Mr. A. R. Colquhoun, for his travels in China, and Dr. Julius von Haast, for his systematic exploration of the southern island of New Zealand, the Royal Medals; Mr. W. W. M'Nair, the Murchison Grant; Mr. Emil Boss, the Bach Grant; Mr. W. O. M'Ewan, the Cuthbert Peek Grant; and Dr. Haast, Dr. Max Buchner, and M. Ferdinand de Lesseps as honorary corresponding members.

GERMANY has been prompt in acknowledging the services of its Cholera Commission; by acclamation a bill was passed by Parliament on Tuesday awarding a sum of 135,000 marks to Dr. Koch and his companions.

THE Council of the British Association have resolved not to entertain any more applications for membership prior to the Montreal meeting, when members and associates will be elected as at any other meeting. The number of tickets applied for is 722.

DURING the discussion in the Dominion House of Commons upon the vote of 25,000 dollars to defray the expenses of the meeting in Montreal in August next of the British Association, some further arrangements for the reception of members were made known. The excursion to the Rocky Mountains will, it is announced, take place on September 4, the members being taken by the New Canadian Pacific Lake route, where specially-constructed steamers make direct connection with the railway on each side. The excursion will probably occupy two weeks, and arrangements have been made that members of the party may not be put to greater expense than one dollar and a half per diem during the trip. Of the 25,000 dollars granted by the Dominion Parliament, 5000 will be used to defray the expenses of the meeting itself, and a fund is being raised to guarantee the Association against loss in connection with the publication of their proceedings. In addition to the Rocky Mountains excursion, excursions will be arranged to Ottawa, Quebec, and probably to Belœil Mountain, a locality of great geological interest. Active preparations are being made at Montreal, Toronto, and other places which will be visited, to give the members a due reception. It has also been arranged by the Associated Atlantic Cable Company that social cable messages to and from the delegates and their friends shall be sent free of charge. This is regarded as a considerable contribution towards the success of the meeting in Montreal.

THE French Association for the Advancement of Science meets at Blois this year from September 4 to 11.

PROF. HUXLEY was examined on Tuesday before the Select Committee of the House of Commons on the Education, Science, and Art Departments. He stated that in his opinion greater attention should be paid in our public schools to physical science. The Endowed Schools Commission was appointed to a great extent on account of the general state of apathy which existed in connection with the endowed schools. There is a distinct provision that a certain proportion of marks should be given to science and modern languages. The system, however, is not well carried out in the public schools; not more than two hours a week are given to science. There is no doubt, Prof. Huxley stated, that the Oxford and Cambridge School Examination Board Regulations tend to handicap science extremely. The examiners found their examinations on what is taught in the schools, and the schools found their instruction on the requirements of the examiners. He regarded the present system of education as wrong from top to bottom. The subjects on which most stress is laid are really luxuries, while those which are regarded as luxuries are really the most essential. The present system of education in the country shuts out young men from many employments for which they should be eligible, and tends to the employment of foreigners. Prof. Huxley thought that an influential Minister, with a seat in the Cabinet, might do a great deal to improve education. It would be his business to judge in what direction the educational system was tending, and to enforce on the educational bodies a modification of their system in the desired direction. He would give the Minister power to insist upon more time being given to science and modern languages.

THE Royal Society *conversazione* on the 7th inst. was well attended, and the exhibits, mainly connected with physical science, interesting. Mr. J. Wimshurst exhibited the continuous electrophorus. This instrument consists of two glass disks, revolving in opposite directions upon the same axis. To the outer faces of the disks radial metallic sectors are attached, which in their turn are touched by brushes of fine wire. It is self-exciting under almost any condition of atmosphere, parts freely with its electricity, and the current will not change its direction while the instrument is at work. In Room II. was shown a map of the earthquake in Essex (April 22, 1884), with photographs of damaged buildings. In Room III. Messrs. Elliott Brothers showed some electrical and magnetic apparatus, including a simplex repeater board, Wheatstone's transmitter, Wheatstone's perforator (new form), portable electric lamps, worked by Skrivanow batteries, the Kew Committee magnetometer, and a selection of electrical and other instruments. In the principal library was a compound magnet, with bifilar suspension, showing the change in total moment produced by dividing a magnet into short lengths, exhibited by Mr. R. H. M. Bosanquet. The magnet consists of eighteen pieces of hard steel which, fitted end to end, form a cylindrical bar. These can be placed in the suspension tray either as a bar, or dispersed as separate pieces. When placed together as a bar the moment is between seven and eight times as great as when the pieces are separated. The equilibrium position of the suspended tray is east and west. When a magnet is placed in the tray a deflection takes place towards north and south. The tangent of this deflection measures the moment, according to the ordinary principles of bifilar suspension. Mr. Hilger exhibited various spectroscopes and a 6½-inch achromatic object glass in a mount of new construction. A new photometer and Dworak's sound radiometer, were sent by Mr. Preece; and Hughes' magnetic balance, and Prof. Minchin's absolute sine electrometer, by Mr. Groves. An interesting apparatus for the generation and distribution of ozonised air, to be placed in the Hôtel-Kursaal de la Maloja, Upper Engadine, was exhibited by Dr. A. Tucker Wise. To render the air as pure as possible in this building, ozone is added to the internal atmosphere in connection with the general plan of ventilation. By means of valves this ozonised air can be turned into any room at the rate of from 60 to 100 cubic metres per hour for each occupant. The Rev. S. J. Perry exhibited a selection from the series of drawings of the solar surface made at the Stonyhurst Observatory from 1880 to 1884.

AT the next meeting of the Society of Telegraph Engineers and Electricians, to be held on Thursday, May 22, at the Institu-

tion of Civil Engineers, Mr. W. H. Preece, F.R.S., will give a review of the work done by the Electrical Congresses of Paris, and will describe the new units determined upon.

THE Lord Lieutenant of Ireland has appointed the following Commissioners to inquire into the management of the Queen's Colleges and Royal University in Ireland:—R. P. Carton, Q.C., Mr. George Johnstone Stoney, F.R.S., Rev. Dr. Gerald Molloy, Rector of the Catholic University, Mr. Wm. Jack of Glasgow University, and Surgeon-General Marsten. Mr. N. D. Murphy, barrister, is appointed secretary.

THE Engineering Department of the Yorkshire College at Leeds is about to be considerably enlarged, to admit of more students at the classes, and towards this object Sir Andrew Fairbairn, M.P., and Sir John Hawkshaw have each contributed 1000*l*.

THE steamer *Alert*, one of the vessels to be engaged in the Greely Relief Expedition, sailed from New York on May 10.]

THE eminent Swedish astronomer, Prof. H. Gyldén, at present chief of the Stockholm Observatory, has been called to the Professorship of Practical Astronomy at Göttingen.

HERR AUGUSTIN GAMÉL of Copenhagen has offered to despatch the *Dijmphna*, under Lieut. Hovgaard, to Franz-Josef Land in the summer of 1885, provided the Danish Government will contribute part of the expenses. No contribution will be accepted from foreign nations.

PROF. NORDENSKJÖLD has executed a detailed map of that part of the east coast of Greenland which he visited last summer, situated beyond Cape Dan, known from Lieut. Graah's journey. The peninsula on which the cape is situated he has named "King Christian's Island," and the harbour in which he landed "King Oscar's Harbour." Several other points have been named after celebrated Swedes and Danes.

THE Finnish Senate has petitioned the Czar that all members of the forthcoming hydrographical expedition in the Baltic, which will cost about 100,000 marks, shall be Finnish subjects, as so little progress with such labours seems to be made under Russian naval officers.

THE St. Petersburg Horticultural Exhibition and Botanical Congress, which was deferred last year owing to the coronation of the Czar, was opened on May 5. Mr. H. J. Elwes, F.L.S., of Preston House, Cirencester, was requested by the Science and Art Department to attend as the delegate on behalf of this country, and has been accredited.

MR. J. E. MARR, M.A., Fellow of St. John's College, Cambridge, has been appointed by the Council to lecture in geology.

A VIOLENT shock of earthquake, having an undulatory character, was felt at Spoleto at 8 o'clock on Friday night. The bells were set ringing and the clocks stopped. On Saturday, at 9.50, a slight shock in the direction of from north-west to south-east was felt at Cosenza, and at Paola a somewhat stronger shock was felt.

THE additions to the Zoological Society's Gardens during the past week include a Barbary Ape (*Macacus inuus* ♀) from North Africa, presented by the Countess of Craven; a Moufflon (*Ovis musimon* ♂) from Sardinia, presented by Col. Knox and the Officers of the 1st Battalion Scots Guards; a Common Raccoon (*Procyon lotor*, white variety) from North America, presented by Mr. F. J. Thompson; a Ground Hornbill (*Buceros abyssinicus*) from West Africa, presented by Capt. Rupert La T. Lonsdale; a Gold Pheasant (*Thaumalea picta* ♀) from China, presented by Mr. Frank Reed; two Peregrine Falcons (*Falco peregrinus*), European, presented by Lieut.-Col. Drummond Moray; two Alligators (*Alligator mississippiensis*) from the Mississippi, presented by Mrs. Andrade; a Green Tree Frog (*Hyla arborea*), European, presented by Mr. G. W. Obicini, F.Z.S.; twenty-one River Lampreys (*Petromyzon fluviatilis*) from British rivers, presented by Mr. T. E. Gunn; two Japanese Greenfinches (*Ligurinus sinicus* ♂ ♀) from Japan, two Common Crowned Pigeons (*Goura coronata*) from New Guinea, purchased; a Canadian Porcupine (*Erithizon dorsatus*), five Long-fronted Gerbilles (*Gerbillus longifrons*), two Variegated Sheldrakes (*Tadorna variegata*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN

THE APPROACHING RETURN OF OLBERS' COMET.—Now that the comet of Pons is drawing away from us, attention may be directed to another comet belonging to the same group as regards length of revolution, viz. that discovered by Olbers at Bremen on March 6, 1815, and last observed by Gauss at Göttingen on August 25. While it was still under observation an elliptic orbit was assigned by several astronomers, including Bessel and Gauss, who found the period between seventy and eighty years. Bessel subsequently discussed all the observations available to him, in a memoir published in the *Transactions of the Berlin Academy*, and, after determining the most probable orbit in 1815, he calculated the planetary perturbations to the time of ensuing return to perihelion, which he fixed to 1887, February 9, the effect of the perturbations being to accelerate the return by about 825 days.

An elaborate investigation of the elements of Olbers' comet and the effects of planetary attraction during the current revolution has been lately made by Herr F. K. Ginzel, of Vienna: it gained the prize of the Haarlem Society of Sciences, and was published by the Society in 1881. The author has availed himself of the improved values of the planetary masses and the other advantages which the astronomy of the last seventy years has placed in our hands, and has produced an interesting and skilfully-worked discussion of the motion of the comet since it passed out of view in 1815. He commences with a solar ephemeris, and coordinates X, Y, Z, founded upon Leverrier's Tables, and extending from March 4 to August 27, followed by an ephemeris from Bessel's ellipse of the comet's geocentric right ascension and declination and log. distance for the same period. In the next section the observations are as far as possible newly reduced, great care and trouble having been bestowed on the determination of the places of the comparison stars from the most reliable catalogues. It may be mentioned that there are observations at fourteen observatories, including Greenwich, where the comet was followed from May 22 to July 7; the series newly reduced are those of Berlin, Göttingen, Königsberg, Paris, Prague, and Seeberg. The necessary data for reduction of mean to apparent places of the comparison stars follow. The effect of parallax is applied to the comet's observed positions, and we have then the entire collection of deduced geocentric places, with the Berlin mean times of the observations.

In Bessel's investigation 187 observations were utilised; Ginzel has the greatly increased number of 346. The perturbations of Mercury, Venus, the Earth, Mars, Jupiter, Saturn, Uranus, and Neptune are next calculated for the period over which the observations extend; twelve normal positions are formed, and cleared of the effect of planetary attraction; then, in the usual manner, Bessel's elements are corrected by equations of condition, and the following definitive orbit for 1815 is obtained:—

Perihelion passage, 1815 April 26·030146 Berlin M.T.

Longitude of perihelion	149°	$2'$	$2''\!\cdot\!8$	M.Eq. 1815·0
,, ascending node	83	28	46·7	
Inclination	44	29	50·8	
Eccentricity	0·93114958			
Log. perihelion distance	0·0837998			

Hence there results a revolution of nearly 74 years. The limits of uncertainty in this period are then examined, and found to be 75·68 and 72·33 years, and thus Ginzel concludes that the time of revolution given by the complete discussion of the observations of 1815 is in doubt to the extent of 1·6 year, or about a year and seven months.

In the next section of the memoir are presented the details of the laborious work involved in the calculation of the effect of planetary attraction during the actual revolution. The separate effect of each of the planets Jupiter, Saturn, Uranus, and Neptune

is assigned, and as regards the period, will be found from Ginzel's numbers to be—

	days
For Jupiter	– 799·16
Saturn	– 27·27
Uranus	– 7·53
Neptune	– 2·76

These figures show a total acceleration of 836·72 days, and hence the most probable epoch of the next perihelion passage is found to be 1886 December 16·9 G.M.T.

After remarks upon the physical observations made in 1815, and Bessel's observation of a nearly central occultation of a star by the comet on April 26, we have extensive sweeping ephemerides to facilitate the rediscovery at the approaching return; the places are given for every tenth degree of the sun's longitude, and of the true anomaly from $-120°$ to $+120°$. In view of the uncertainty in the length of the comet's period, it may be well to commence the search in 1885.

In a supplement Ginzel examines the effect of the attraction of the smaller planets Mercury, Venus, the Earth, and Mars from March 1815 to February 1817; also the possible effect of a resisting medium: these are found to be too small to be worthy of consideration practically.

The elements assigned by Ginzel's investigation for the comet's next appearance are:—

Perihelion passage, 1886 December 16·9338 Berlin M.T.

Longitude of perihelion	149° 48′ 40″·3	M. Eq. 1887·0
,, ascending node... ..	84 31 24·2	
Inclination	44 33 34·3	
Angle of eccentricity	68 31 3·0	
Mean daily sidereal motion	49″·387785	
Log. semi-axis major	1·2375914	

To which corresponds a period of revolution of 71·843 years.

THE BUILDING OF THE ALPS [1]

II.

I PASS now to the section of the Simplon. On the southern side, deep in the glen of the Doveria, in the vicinity of the gorge of Gondo, we find a mass of granitoid gneiss, which recalls to mind that already described from the wildest portion of the upper valley of the Reuss. We may, I think, with confidence affirm that, whatever be the true nature of this rock, we are again touching the foundation-stones of the rock masses of the Alps. As we approach Algaby, the granitoid gneiss becomes more distinctly bedded and variable, a thin band of micaceous crystalline limestone is passed, and presently the more rapid ascent of the pass begins. Hence to beyond the summit we traverse, so far as can be seen, a great series of bedded gneisses, often coarse and even porphyritic, and of schists. The same are displayed in the crags of Monte Leone on the east and of the Rossbodenhorn on the west. As shown in Prof. Renevier's valuable section, bands of crystalline dolomitic limestone, and of hornblendic and garnetiferous schists occur in various places on either side of the Simplon road. Then, after descending about half way to Brieg, we strike the group of the Lustrous Schists, with the usual calcareous zone in the lower part. Prof. Renevier does not attempt to unravel the complexities of the strata which compose this portion of the central ridge of the Alps, and I feel that my slighter knowledge makes caution yet more imperative; but I think we are justified in asserting that we have evidence of an upward succession from the coarse granitoid fundamental gneisses, through more variable and bedded gneisses, to a group which recalls the garnetiferous schists, so finely developed on the southern flanks of the St. Gothard—a group also traceable in the upper portion of the Binnenthal, though apparently far less perfectly developed. I think also that in the gigantic anticlinal of the Simplon we have evidence of sharp flexures on a great scale; and that these garnetiferous schists are only here and there preserved as the lower ends of infolded loops, so that the bulk of the *massif*, and, so far as I can tell, the actual summit ridges of the Rossbodenhörner and Monte Leone, are composed of the bedded gneisses and strong schists, and perhaps of the more friable gneisses which have been already described in the mountains further to the east.

The mountains further west—the aspiring peaks which rise around the two branches of the Visp, including among them some of the highest summits of the Alps, such as Monte Rosa, the Mischabelhörner, the Matterhorn, and the Weisshorn—offer indeed magnificent sections, but are full of difficulty. The fundamental gneiss, if I mistake not, is occasionally exposed—as, for example, in the rocks of Auf der Platte, at the base of Monte Rosa; and in parts of the Mischabelhörner blocks of coarse granitoid rock, often very porphyritic, which I refer to the same series, are brought down by the glaciers. There are also mica schists in plenty, such as the summit rocks of Monte Rosa and the backbone—if the phrase be permitted—of the Mischabel- and Saaser hörner, which I refer to the second zone already described—that of the bedded gneisses and strong mica schists. I have also seen specimens which closely resemble the garnetiferous schists of the St. Gothard district, but we meet in this district with a group of rocks which, if not altogether unknown before, appear now to be developed to an exceptional extent, and to become an important factor in the Alpine crystalline series.

Those who are familiar with the environs of Saas and Zermatt will remember how frequently schists or schistose rocks of a greenish colour occur. Sometimes they are interbedded with strong mica schists, or schisty quartzites, sometimes they form homogeneous masses of considerable extent. It is possible that some of the latter are intrusive masses of serpentine, to which subsequent pressure has given a schistose aspect; certainly there are occasional masses of coarse gabbro, which I think undoubtedly an intrusive igneous rock; but still, making all allowance for such cases, there is in this region a considerable mass of greenish hornblendic, talcose, and serpentinous rocks which appears to be non-igneous in origin. We find these all around Zermatt. They form the ridges of the Gorner Grat and of the Hornli. They break out through the snows of the Breithorn and Little Mont Cervin, and constitute no inconsiderable portion of the mighty obelisk of the Matterhorn. The whole of that peak, according to the investigations of Sgr. Giordano—and with this my own recollections correspond—consists of an apparently regularly bedded series of serpentinous and micaceous schists, and of greenish gneisses, with the exception of a gabbro, developed on the western side, which I have no doubt is an intrusive rock. Can we trust these indications? Are we justified in assigning to this zone, with those characteristics, a vertical thickness of more than a mile? To these questions I can give at present no answer, further than to state that I am convinced that, notwithstanding the apparent regularity of the bedding in this and the neighbouring peaks, there are really great folds which patient scrutiny may at length unravel, and that this zone of greenish rocks—for which Alpine geologists have proposed the name of *Pietra Verde* group, appears to underlie the garnetiferous series of silvery mica schists, and either to overlie or replace the upper portions of the banded gneiss series which succeeds to the fundamental series.

I do not propose to weary you further with the details of Alpine sections, except that I must add a few words upon the extent of this remarkable series to which I have now introduced you. On the northern side of the watershed in the Swiss Alps, so far as I am aware, it is not generally strongly developed, except in certain localities in the southernmost of the three ranges which make up the whole chain, but in parts of the Tyrol it is well displayed. It borders—the mica schists sometimes dominating—the fundamental gneiss in the Oetzthal *massif;* it forms the peak of the Gross Glockner; it meets us on the Brenner Pass and elsewhere overlain by and folded up with rocks which, if my memory do not mislead me, are the equivalents of the Lustrous Schists of more western districts.

Again, it is finely developed, seemingly in succession to bedded coarser gneiss, in some of the peaks of the Bernina range, and it occupies a considerable tract about the heads of the valleys to the south. It may be traced, indeed, over a great zone, and with but slight interruption all along the southern slopes of the Alps, even to the south of the head waters of the Po, forming many of the grandest peaks in the Graian, Tarentaise, Maurienne, and Cottian Alps; and we find traces of it overlying the coarse granitoid series in the *massif* of the Alps of Dauphiné.

Sections, indeed, in the neighbourhood of Biella, according to Gastaldi and Sterry Hunt, exhibit the Pietra Verde group overlying the upper or more bedded portion of the great gneissic or basal series, and succeeded by the group of friable gneisses, described above as closely associated with the garnetiferous schists, in a manner that suggests an unconformity. Under ordinary circumstances we should not hesitate to admit

[1] Lecture by Prof. T. G. Bonney, D.Sc., F.R.S., Pres.G.S., at the Royal Institution, April 4. Continued from p. 46.

that there is considerable evidence in favour of this break, and some for one between the Pietra Verde group and the stronger gneisses and schists below; but in mountain regions we fear to trust our eyes. The evidence, however, in certain districts in favour of a break at the base of the Lustrous Schists is yet stronger. If I am right in regarding the Lustrous Schists as forming one group with the older part of the Bundnerschiefer of the Grisons region, and of the Thonschiefer of Von Hauer in the Eastern Alps, a study of the geological map will show that it is difficult to explain the relation of these beds to the underlying gneisses and schists without such an hypothesis. What I have myself seen in regard to the Lustrous Schists is strongly in favour of a great break in some localities. On the south side of the St. Gothard we have in the Val Piora the Lustrous Schists apparently in true succession with the representatives of the garnetiferous group of the Val Tremola, yet on the northern side, in the Urserenthal, the latter series is wanting, and the gneisses which underlie it appear to be immediately succeeded by the Lustrous Schists. This, however, might be explained by a complication of faulting and folding. What I have seen in the Binnenthal is harder to explain. At the head of the Hohsand Glacier, just below the peak of the Ofenhorn, we have a coarse but bedded gneiss, which I should correlate with the series immediately overlying the granitoid gneiss so often mentioned as the lowest rock of all. Glancing towards the north, across the snowfield, we see this rock in the base of the Strahlgrat distinctly overlain by the Lustrous series, with its characteristic band of limestone or dolomite. This series swoops down for some 2000 feet, and we cross it in the upper basin of the valley below, while yet further down the valley I detected the characteristic garnetiferous schist, of which, however, there is no great development. If this be the result of faulting and folding only, it is certainly very remarkable.

But I must linger no longer over details. The passing time warns me that I must attempt briefly to describe the general process of the building of this great mountain group of Europe. I have, I hope, proved that the metamorphic rocks of the Alps, if we may trust mineral similarity and mineral and lithological sequence, are vastly older than the Carboniferous period, and that in this ancient series a certain succession may be made out. If we may reason from the analogy of other regions, we may assign to the whole of their latest group (the Lustrous Schists) an antiquity greater than the earliest rocks in which indisputable traces of organic life have been found. One point, however, I should notice before proceeding further. It might perhaps be said—it has indeed been said—that the crystalline schists and gneisses of the Alps are the result of the great earth movements by which the mountains were upraised, when heat and pressure changed mud into schists and felspathic sandstone into gneiss. I have shown you that we can trace a mineral succession in the crystalline series of the Alpine chain, and that some at least of these are earlier than the Carboniferous period; but I can add to the proofs that these great rock masses had assumed in the main their present mineral structure when these movements occurred. We meet with some rock masses whose structure is doubtless due to the pressure which they have undergone. This is the case with all cleaved rocks, as was lucidly explained twenty-eight years since by Prof. Tyndall in this very room. We meet also with schists, where, from the arrangement of the mineral constituents, we have good reason for supposing that they were developed when the rock mass was exposed to a pressure definite in direction. Here the lines of different minerals, which we believe indicative of an original structure in the rock, are often wrinkled; the more flaky minerals commonly lie with their broader planes parallel, but, notwithstanding this, there is no very definite cleavage in the rock mass, nor tendency to separate easily along the different mineral layers. Specimens of such rocks may be obtained in the Alps, but there are others in which the layers have evidently been crumpled up after the period of mineral change: the bands of quartz and felspar have been, as it were, crushed together, the flakes of mica are sometimes crumbled and sometimes twisted round into new positions.

The subject is a technical one, so I must ask you to accept my statement, without the long details of microscopic work on which it is founded, that the older Alpine rocks frequently testify to having undergone an extraordinary amount of crushing. In the middle of coarse gneisses, for example, streaks and thin bands of a mica schist may be found, which are not due to an original difference of materials, but to the fact that here and there the original rock has yielded to enormous pressure, and has been crushed *in situ* into lenticular bands of rock dust, from which some new mineral developments have taken place. You may notice also in some regions, where you would classify the rocks at first sight as mica schists, that a close examination of the broken surfaces at right angles to what appear to be planes of foliation reveals a structure resembling a coarsish gneiss. The microscope shows that the rock is really a gneiss, somewhat crushed, and that the micaceous layers are of extreme tenuity—mere films, which do not seem to have been original constituents. The gneissic mass has been crushed, cleaved, and on the cleavage planes films of a hydro-mica have been developed. We cannot fail to be struck, when once our eyes have been opened to it, by the frequency of a slabby structure in the more central parts of the Alpine ranges, the surfaces of these slabs being coated with minute scales or films of mica. These are really records of a rude cleavage which has been impressed upon the more central and less flexible portions of the Alps during the great earth movements which they have undergone since they were first metamorphosed.

Thus in the building of the Alps our thoughts are carried very far back in the earth's history, far beyond the earliest strata of the Palæozoic age. Under what conditions were these great homogeneous granitoid masses of the fundamental gneisses formed? They differ on the one hand from granites, on the other from the ordinary gneisses; from the former their differences are but slight, and of uncertain value, yet into the latter they appear to graduate. There is nothing like to them in any subsequent rock group, and, so far as our knowledge at present goes, they appear to be the records of a period unique in the world's history. This may well be. When the dry land first appeared, when the surface of the earth's crust had not long ceased to glow, when the bulk of the ocean yet floated as a vapour in the heated atmosphere, when many gases now combined were free, we can well imagine that the earliest sediments would be deposited under conditions which have never been reproduced. In the later schists, with their more frequent mineral changes, their distinct stratification, and their beds of quartzite and of limestone, we may mark the gradual approach to a more normal condition of things. Some, such as the Lustrous Schists, may indeed be contemporaneous with our earliest Palæozoic rocks; but I confess that to myself the evidence appears more favourable to the idea that all are more ancient than the period which we call Cambrian, and that the majority are so I feel little doubt.

Supposing, then, that I am right in considering all the Alpine schists, even the Lustrous group, to be pre-Cambrian, we have a vast interval of time which has left no record in those districts of the Alps of which I have been speaking. It is not till we come to the Carboniferous period that we can identify any pages in the life-history of the earth. We are justified with regard to these in the following conclusions:—

That in the place of the Alps there was at that time an upland district, composed of gneisses and schists, in substantially the same mineral condition as they are at present, together with slaty beds in a comparatively unaltered condition, which district was fringed by a lowland covered by a luxuriant vegetation. Prior to this time, also, the metamorphic rocks of the Alps had been so far folded and denuded that the coarser gneisses were in many places laid bare, and contributed the materials which we now find in such beds as the Val Orsine pudding-stone. Whether there was a pre-Triassic mountain chain occupying some parts of the present Alpine region we cannot venture to say, but I think we may unhesitatingly affirm that there were pre-Triassic highlands.

After the close of the Carboniferous period, and anterior to the middle part of the Trias, there were volcanic outbursts on a large scale in more than one region of the Alps—notably in the district near and to the east of Botzen. After this commenced a period of subsidence and of continuous deposition of sediment. This seems to have begun earlier and to have been at first more rapid in the eastern than in the western area. Since in the former the Triassic beds are generally much thicker and more calcareous than in the latter, one is tempted to imagine that the eastern area quickly became a coraliferous sea, with an occasional atoll or volcanic island. Henceforward to the later part of the Eocene the record is generally one of subsidence and of deposit of sediment. Pebble beds are rare: the strata are grits, shales (or slates), and limestones. Whence the inorganic constituents of these were derived I cannot at present venture to suggest, but though conglomerates are rare, there are occasional indications that land was not very distant. In the eastern Alps, however,

the position of some of the Cretaceous deposits and the marked mineral differences between these and the Jurassic seem to indicate disturbances during some part of the Neocomian, but I am not aware of any marked trace of these over the central and western areas. The mountain-making of the existing Alps dates from the later part of the Eocene. Beds of about the age of our Bracklesham series now cap such summits as the Diablerets, or help to form the mountain masses near the Tödi, rising in the Bifertenstock to a height of 11,300 feet above the sea. Still there are signs that the sea was then shallowing and the epoch of earth movements commencing. The Eocene deposits of Switzerland include terrestrial and fluviatile as well as marine remains. Beds of conglomerate occur, and even erratics of a granite from an unknown locality, of such a size as to suggest the aid of ice for their transport. For the present I prefer, for sake of simplicity, to speak of the upraising of the Alps as though it were the result of a few acts of compression, though I am by no means sure that this is the case. Thus speaking we find that in Miocene times a great mountain chain existed which covered nearly the same ground as the present Alpine region of Mesozoic and crystalline rocks. To the north, and probably to the south, lay shallow seas, between which and the gates of the hills was a level tract traversed by rivers, perhaps in part occupied by lakes. Over this zone, as it slowly subsided—in correspondence, probably, with the uplifting of the mountain land—were deposited the pebble beds of the nagelflue and the sandstones of the molasse.

Then came another contraction of the earth's crust; the solid mountain core was no doubt compressed, uplifted, and thrust over newer beds, but the region of the softer border land, at any rate on the north, was apparently more affected, and the sub alpine district of Switzerland was the result. I may here call your attention to the fact that, whether as a consequence of this or of subsequent movements, the Miocene beds occur on the northern flank of the Alps at a much greater height above the sea than on the southern, and have been much more upraised in the central than in the western and eastern Alps. Further, between the Lago Maggiore and the south of Saluzzo, Mesozoic rocks are almost absent from the southern flank of the Alps, and the Miocene beds are but slightly exposed and occupy a comparatively lowland country. I think it therefore probable that the second set of movements produced more effect on the German than on the Italian side of the Alps, producing on the latter a relative depression. In support of this we may remark that the rivers which flow from the Alps towards the north or the west, start, as a rule, very far back, so that the watershed of the Alps is the crest of the third range reckoning from the north, and the great flat basin of the Po is the receptacle for a series of comparatively short mountain rivers. These also take a fairly straight course to the gates of the hills, while the others change not seldom from the lines of outcrop to the lines of dip of the strata—a fact I think not without significance. To this rule the valley of the Adige in the eastern region is an exception. May not this be due to the remarkable series of minor flexures indicated by the strike of the rocks (Mesozoic and earlier) immediately to the west of it, which probably influences the course of the Adda, and can, I think, be traced at intervals along the chain as far as Dauphiné? Be this as it may, it is obvious that the generally uniform east-north-east to west-south-west strike of the rocks which compass the Alpine chain is materially modified as we proceed south of the Lake of Geneva, changing rapidly in the neighbourhood of Grenoble from a strike north-east to south-west, to one from north-west to south-east. This subject, however, is too complicated to be followed further on the present occasion. I will only add that the singular trough-like upland valleys forming the upper parts of some of the best-known road passes—as, for instance, the Maloya—which descend so gently to the north, and are cut off so abruptly on the south, seem to me most readily explained as the remnants of a comparatively disused drainage system of the Alps.

It remains only to say a few words on the post-Tertiary history of the Alps. We enter here upon a troubled sea of controversy, upon which more than the time during which I have spoken might easily be spent; so you will perhaps allow me to conclude with a simple expression of my own opinion, without entering into the arguments. That the glaciers of the Alps were once vastly greater than at the present time is beyond all dispute; they covered the fertile lowlands of Switzerland, they welled up against the flanks of the Jura above Neufchatel, they crept over the orange gardens of Sirmio, and projected into the plains of Piedmont. By their means great piles of broken rock must have been transported into the lowlands; but did they greatly modify the peaks, deepen the valleys, or excavate the lake basins? My reply would be, "To no very material extent." I regard the glacier as the file rather than as the chisel of nature. The Alpine lakes appear to be more easily explained—as the Dead Sea can only be explained—as the result of subsidence along zones roughly parallel with the Alpine ranges, athwart the general directions of valleys which already existed and had been in the main completed in pre-Glacial times. To produce these lake basins we should require earth movements on no greater scale than have taken place in our own country since the furthermost extension of the ice-fields. This opinion as to the origin of the lakes is, I believe, generally held to be a heresy, but it is a heresy which has been ingrained in me by some twenty years of study of the physiography of the Alps.

RECENT MORPHOLOGICAL SPECULATIONS

I.—*On Alternation of Generations*

IT is more than sixty years since Chamisso pointed out that in Salpa a solitary asexual individual produced a chain of sexual individuals by budding, the viviparous eggs in these becoming in turn the solitary form. This he made his type of *Alternation of Generations*.

Since his time the definition of this peculiar method of reproduction has been narrowed, and the alternation of a series of individuals developed from an unfertilised egg, *i.e.* parthenogenetically, and one or more generations of sexually produced young is now called *heterogamy;* the term *metagenesis* has been invented for cases of alternation of sexual and gemmiparous generations.

Few instances can be cited where the study of a single genus has brought out so many points of interest as in the case of the pelagic Ascidian, Salpa. Two points in the history of this animal still involved in controversy are the first phenomena of embryonic development, and the mutual relationship of the two forms, the solitary individual and the colony that swim united in a chain.

As regards the former matter, the fate of the egg and the origin of the nutritive structure known as the placenta present great difficulties.

While W. K. Brooks (*Bull. of Museum of Comp. Zool.*, Harvard University, iii.) believed that the egg undergoes a regular segmentation resulting in the formation of a gastrula, the cavity of which is divided by a transverse constriction into two chambers, one becoming the "placenta," Todaro (*Atti della R. Accad. dei Lincei*, Rome, 1875, 1880), on the other hand, stated that the solitary Salpa is the result, not of the division of the true ovum, but of the *follicular cells* inclosing it, and that during development, which takes place in a special part of the oviduct, the so-called uterus, a fold of the uterine wall forms a decidua reflexa comparable to that of mammals.

Salensky (*Zool. Anzeiger*, 1881; *Mittheil. d. zool. Stat. zu Neapel*, Bd. iv.) accounts for some of these conflicting statements by showing that great variety exists in nearly allied species, but he also declares that previous observations were often inaccurate. He states that the fertilised ovum divides but slowly, and only till the number of its segments reaches sixteen; and that probably it then entirely disappears, the tissues of the embryo being formed from a varying number of *follicular cells*. In some cases, as *S. bicaudata*, the so-called "decidua" is not represented. To this most exceptional method of development he gives the name of "follicular budding."

Now the theory that Salpa is an instance of the alternation of sexual and gemmiparous generations (*i.e.* of *metagenesis*), which was put forward by Chamisso and supported by the researches of Krohn, has been attacked by Brooks, who believes that the solitary Salpa (which he calls the *nurse*) is hermaphrodite, and gives rise by budding to a chain of males into each of which an egg migrates from the nurse. This view of course supposes that the solitary and chain forms belong to the same generation, they being, in fact, respectively the sexually and asexually produced offspring of one and the same solitary hermaphrodite Salpa. Todaro, on the other hand, states that, in the solitary adult, certain of the follicular cells surrounding the ovum give rise to no organs, but remain as cell-masses; and that from these the stolon is eventually developed. Hence the chain-Salpæ are developed parthenogenetically, and the nurse is an older sexless form, serving to nourish the sexually complete chain.

In Brooks' theory the main point is the migration of ova from the solitary individual into the individuals of the chain. In the light of a study of closely allied genera we find serious objections to this view. The fact on which it is chiefly based is that in the stolon when quite immature, we can trace the following organs: *a*, the outer tunic or epidermis; *b*, the pharyngeal cavity continuous with the pericardium; and *c*, two "club-shaped masses" of cells which lie on either side of *b*, and which soon resolve themselves into two lines of ova, one of which passes into the sinus system of each zooid. The discovery of undoubted ova in the stolon when the organs of the zooids are hardly indicated suggests, says Brooks, a migration from the nurse, which is therefore female.

Now we have, in direct opposition to this, an observation of Salensky's, that in some cases a second ovary is developed in the chain-Salpæ; and an indirect negation is entailed in the facts of gemmation in Pyrosoma, which is generally allowed to be a less modified form. Here we find the bud, when merely a protuberance on the mother, consisting of an epidermis derived from that of the mother, an "archenteric" cavity continuous with the endostyle, and a mass of cells which are derived directly from the "generative blastema." In this mass a single ovum can be seen quite as early relatively as in Salpa, and a second near the base supplies the secondary bud. Among the Composite Ascidians the case is similar; in Amourœcium the buds are cut off from the post-abdomen and consist of outer tunic, mesentery (that is, continuation of pharynx backwards), and two lateral masses in which germinal vesicles shortly appear; in Didemnium, also, although Kowalevsky traced the buds back to a condition much more nearly resembling the segmenting ovum, still even here the single ovum is one of the most conspicuous of the primitive organs. It is apparent that there is in the Tunicata a tendency to form buds at the expense of the three primitive layers, and that some advantage attends the early development of ova. Whether this is to avoid the danger of self fertilisation or not, it reaches its limit in Salpa, where the rudiment of the ovary only consists of one fully developed ovum.

A similar modification in the time of development of the ova has taken place in some of the Hydrozoa (Hydrella, Sertularia, &c.), where, as Weismann has pointed out (Abstract and Review by Prof. Moseley in NATURE, vol. xxix. p 114), the immature ova may be detected in the cœnosarc before even a rudiment of the bud appears.

The view which Todaro upholds seems also to be negatived by Salensky's observations. For if the solitary Salpa is developed by follicular budding, it is not remarkable that some of the cells should form an organ corresponding to the generative blastema of Pyrosoma, *i.e.* giving rise only to the generative cells.

If Salensky's facts stand the test of further observation, we have in Salpa not only a unique method of development but a unique alternation of generations, namely, of gemmation and parthenogenesis, only comparable to that of the Aecidiomycetes among plants.

In Pyrosoma, the individual developed directly from the egg is the "cyathozooid," and this remains rudimentary, giving rise to the first four ordinary individuals by budding. There is here an alternation of a single asexual form with numerous generations produced by budding, each of which becomes hermaphrodite.

According to Ganin, we have a precisely similar case in the Composite Ascidians, for he states that sexual organs only develop in the individuals produced by gemmation.

In Doliolum the ovum, as before, gives rise to an asexual individual, but the lateral and median buds which arise on its dorsal stolon do not become sexually mature. The former only serve to nourish and protect the latter (Grobben, *Arbeit. der Zool. Insti. zu Wien*, iv.), from which a ventral stolon (stalk of attachment) grows out, bearing sexual individuals.

Outside the group of the Tunicata, true alternation of generation occurs in some Cœlenterates, some Annelids, some Cestodes, and possibly in the Trematodes.

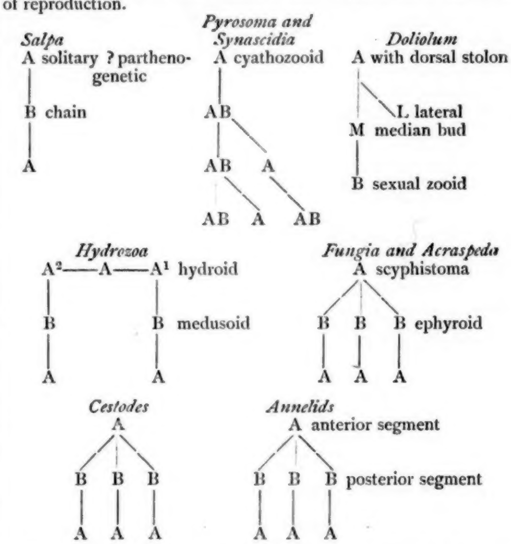
The alternation of hydroid and medusoid forms in many Hydromedusæ (Gymnoblastic and Calyptoblastic Hydroids and Hydrocoralla), all Acraspeda except Pelagia, and possibly Fungia among the Actinozoa, has been dwelt on in a previous paper in this journal by Prof. Moseley, and little range of modification occurs except in the extent of development of the medusæ.

In Cestodes the complicated metamorphosis has been mistaken for metagenesis, but it is only in those cases where the cystic worm develops a number of "heads" by gemmation, *e.g.* *Cœnurus cerebralis*, *Tænia echinococcus*, that metagenesis really occurs.

There is more uncertainty about the condition of affairs in the Trematodes, the ordinary view being that "the majority of the stages are simply parts of a complicated metamorphosis, but in the coexistence of larval budding (giving rise to Cercariæ or a second generation of Rediæ) with true sexual reproduction there is in addition a true alternation of generations" (Balfour, "Comparative Embryology," vol. i. pp. 172, 173). Grobben (*loc. cit.*), however, has lately stated that the Cercariæ are developed not from a mass of cells produced by internal budding in the Redia but from an ovum developed parthenogenetically. This would place these phenomena under the category of heterogamy.

Among the Polychætes there exist in Syllis, Myrianida, and Autolytus undoubted cases of alternation of generations; but these are not of recent discovery, having been described by Quatrefages, Krohn, and A. Agassiz about 1850–1860 (Balfour, *ibid.*, i. pp. 283, 284).

A general comparison of the various ways in which reproduction is carried on within the limits of alternation of generations may be easily made by a series of diagrams in which A represents the asexual individual developed from the fertilised egg, B the sexual zooid, and A B those forms which carry on both methods of reproduction.



Heterogamy, which is not so common as metagenesis, has been the subject of very interesting memoirs by Adler and Lichtenstein. In a paper, "Ueber den Generationswechsel der Eichent Gallwespen" (*Jenaische Zeitschrift*, 1881), we have the result of Dr. Adler's work on "Gall-making Hymenopterous Insects," formerly described as belonging to eight different genera, namely *Neuroterus*, *Aphilothrix*, *Dryophanta*, *Biorhiza*, *Spathegaster*, *Andricus*, *Teras*, and *Trigonaspis*. He confirms the conclusions of Lichtenstein that certain species of the first four of these genera are stages in the life-history of certain species of the last four.

The gall wasps which in April leave the small round scale-like galls on the under surface of the leaves of the oak, have been described as *Neuroterus ventricularis*; but the parthenogenetic egg develops within a round soft "currant-gall" to a wasp named *Spathegaster baccarum*. The latter escapes in June, and differs from the Neuroterus in size, colour of the legs, and in the female in the number of joints in the antennæ. The eggs produced by the Spathegaster when fertilised develop within Neuroterus galls. A still greater difference exists between the two generations formerly called *Biorhiza renum* and *Trigonaspis crustalis*. Trigonaspis is 4 mm. long, winged, almost entirely black, with antennæ of 15 (♂) and 14 (♀) joints, while the Biorhiza is 1·5 mm. long, wingless, red-brown, and with 13 joints in the antennæ; the two forms live, moreover, in different kinds of galls. In all these cases the alternation is direct. But among

other insects several generations produced by parthenogenesis[1] intervene between true sexual generations.

The typical case is that of the Aphides, which has been long known; here there is little difference of habit or structure except as regards wings, and possibly the generative organs.

Some of the Aphides, however, do show modifications in their life-history almost as remarkable as in the case of the Cynipidæ mentioned above. Lichtenstein's researches, though vigorously attacked by some French writers, have been confirmed by Kessler and Horvath. He observed that the Aphides, living during summer at the roots of various grasses, become winged in autumn and fly to the trunks of trees, where they produce sexual individuals; the solitary egg of the female remains dormant in her dried body till the following spring, when it develops into a gall-making aphis, the *foundress pseudogyne*. This produces viviparous winged young (*emigrant pseudogynes*), which in June fly back to the grass, lose their wings, and produce fresh generations by parthenogenetic eggs. This completes the cycle, and the generations distinguished by habitat are often different in appearance, even a large number of different forms being sometimes thus connected. For instance—

Phylloxera quercus (Balbiani) migrates from *Quercus ilex* to *Q. sessiliflora*.

P. vastatrix, from the leaf-galls to the root of the vine.

Tetraneura rubra, from galls on trunk of elm to roots of dog's grass.

T. ulmi, from elm-galls to roots of maize.

Other less perfect examples of heterogamy, such as Cecidomyia and *Ascaris nigrovenosa*, are well known.

The current views concerning the probable origin of the phenomena of heterogamy and metagenesis may be roughly classed in two groups, one formulated by Leuckart and supported by Claus and Balfour, the other held by Salensky and Brooks.

Leuckart supposed that alternation of generations is a division of labour in regard to reproduction in which the two chief kinds of multiplication, sexual and asexual, are divided between different individuals and generations.

The other theory is that these phenomena are due to a modification of metamorphosis, Salensky also stating that "the connection between metagenesis and metamorphosis is much more easily seen in Tunicates than in other animals."

If in an animal undergoing a metamorphosis the larva acquired the power of producing other individuals by budding, we should have the larval form undergoing finally a change into the adult sexual form. At the same time it is obvious that this is not an indispensable condition; for the more individuals the larva produced the more incapacitated it would be for future sexual reproduction, so that in all probability there would soon be no development beyond the larval stage. W. K. Brooks, in a recent paper on the subject, alludes to a medusa, Cunina, the hydroid form of which is parasitic on the stomach of another medusa, Turritopsis. This hydroid produces medusæ by gemmation, but is itself finally modified into a medusa.

The contrast between this and the more usual case is thus:—

Cunina	*Ordinary Forms*
Egg	Egg
{Larva becoming adult} — {Adult}	Hydra——Hydra——Hydra
	Medusa Medusa

Similarly in the Cestodes it is usually allowed that the Echinococcus stage consists in the production of a number of individuals in the larval state, not of adults differentiated to meet diverse methods of reproduction.

In such a form as Doliolum, and indeed generally among the Tunicata, there seems to be more difficulty attending this view. Gemmation does not result in the production of individuals like the gemmating zooid, which by growth become unlike it. The cyathozooid, or the Doliolum with dorsal stolon, not only do not become sexual after a metamorphosis, but they give rise to the ascidiozooid, a form with ventral stolon in no way comparable to the adult of which it is the arrested larva. It seems here more probable that the existence of two methods of reproduction perhaps taking place at slightly different times has led to the selection of two sets of individuals, one better fitted for gemmation, the other for sexual reproduction. We must then suppose that any influence acting for the modification of one generation is transmitted not to the next but the next but one. This is, however, in no way more strange than the transmission of sexual characters.

While, however, the case of the Tunicates may be considered doubtful, it will probably not be denied by unprejudiced observers that the phenomena seen in the Insecta are more easily accounted for by Leuckart's hypothesis than by Salensky's.

It would seem that there could be no ground for saying with regard to the Cynipidæ, that in a group of which it is characteristic that only the mature forms, and sometimes not even those, are winged—one winged form is the larval condition of another which is smaller, but not otherwise very different, yet this is the case in *Dryophanta scutellaris*—*Spathegaster taschenbergi*.

It is probable, then, that these methods of reproduction have not had in all cases the same origin, and, as in several other instances to which attention has been paid during the last few years, the resemblances, which occur in various animals in no way connected but distributed over almost the whole animal kingdom, may best be considered as *homoplastic*, that is as brought about in different ways under the influence of similar conditions of life.

R. N. G.

[1] Huxley's distinction of true ova and pseud-ova appears not to hold.

THE PARIS ACADEMY OF SCIENCES

THE yearly public meeting of this body was held on Monday, May 5, under the presidency of M. Emile Blanchard. The proceedings consisted mainly of a detailed statement of the awards made for prize essays or distinguished services rendered during the year 1883 to the various branches of the mathematical and natural sciences, useful arts, and industries.

In Mechanics the Extraordinary Prize of 6000 francs, established to encourage improvements of all sorts in the efficiency of the French Naval Service was divided, as in previous years, amongst several candidates. For his "Studies on Marine Engines," now in course of publication, M. Taurines received 3000 francs, M. Germain 2000, for his "Treatise on Hydrography," and Capt. A. de Magnac 1000, for his "New Astronomic Navigation," published in 1877. The Montyon Prize, in the same department, was also divided, half going to M. Léon Francq, for his improvements in Lamm's steam traction engine, and half to Capt. L. Renouf, inventor of an instrument intended to simplify the observation of altitudes at sea, dispensing with the necessity of employing artificial horizons and enabling exact calculations to be made without stopping the vessel under sail or steam. M. Jacquemier, inventor of the kinemometer, dynamometer, and other useful appliances, gained the Plumey Prize; and M. Marcel Deprez the Fourneyron, for his ingenious electric experiments on the Chemin-de-fer du Nord.

The Lalande Prize, founded by the illustrious astronomer to stimulate astronomical studies in France and abroad, was unanimously decreed to MM. Bouquet de la Grye, de Bernardières, Courcelle-Seneuil, Fleuriais, Hatt, Perrotin, Bassot, Bigourdan, and Callandreau, chiefs of the various French expeditions sent to observe the transit of Venus on December 6, 1882. In this branch the Valz Prize was assigned to M. Stephan, Director of the Marseilles Observatory, and discoverer of about 700 nebulæ, the positions of over 500 of which he has carefully determined.

For his extensive labours in the field of Experimental Physics M. Henri Becquerel was rewarded with the Lacaze Prize, the only one given away in this department.

In Chemistry the Jecker Prize was secured by M. Etard for his numerous researches and publications on organic chemistry. M. L. Cailletet obtained in this branch the Lacaze Prize for his important researches on the liquefaction of gases, and especially for his success in, for the first time, demonstrating the possibility of liquefying all the so-called permanent gases.

In Geology the Grand Prize granted by the Minister of Finance for the best geological description of any region in France or Algeria fell to M. Fontannes for his long and successful researches in the Tertiary Basin of South-East France, mainly embodied in his "Stratigraphic and Palæontological Studies of the Tertiary Period in the Rhone Valley." An exceptional award of 2000 francs was also made in favour of M. Péron, author of an extremely important work entitled "Essay on a Geological Description of Algeria."

For his comprehensive monograph on Trichinosis, M. Joannès Chatin, Director of the Government Laboratory at Havre, obtained the Barbier Prize; and MM. G. Bonnier and L. Mangin the Desmazières Prize for their memoir on the "Respiration and Transpiration of Fungi," both in the department of Botany. In the same department M. Costantin was the successful competitor

for the Bordin Prize, awarded for the best solution of the following question proposed in 1879 by the Academy:—"Explain by direction observation and experiment the influence exercised by the environment on the structure of the root, stem, and leaves of vegetable organisms. Study the modifications undergone in water by land plants and those experienced by aquatic plants compelled to live in the air. Explain by direct experiments the special forms of some species of marine flora."

In Agriculture the Morogues Prize was gained by M. Duclaux for his great work on "Biological Chemistry," forming part of the "Chemical Encyclopædia" published under the direction of M. Fremy.

In Anatomy and Zoology the Grand Prize granted by the Minister of Finance for the best memoir on the "Histological Development of Insects during their Metamorphoses," as proposed by the Academy, was assigned to the young naturalist, Dr. H. Viallanes, for his "Researches on the Histology of Insects, and on the Histological Phenomena accompanying the post-embryonic Development of these Animals." In the same department the Bordin Prize was awarded to M. Grand'Eury, who, in two memoirs entitled "Carboniferous Flora of the Department of the Loire and Central France," and "On the Formation of Coal," deals satisfactorily with the "Botanical or Zoological Palæontology of France or Algeria," as proposed by the Academy to competitors for this prize.

Subjoined are some of the most important prizes proposed for 1884 and following years:—

1884

BORDIN: General Study of Monge's Problem of Earthworks.

FRANCŒUR: Works or discoveries useful to the progress of the pure and applied mathematical sciences.

THE EXTRAORDINARY PRIZE OF 6000 FRANCS: Studies tending to increase the efficiency of the French Naval forces.

PONCELET: Awarded to the author of the most useful work in advancing the pure or applied mathematical sciences.

PLUMEY: For improvements in steam-engines or any other iuvention contributing most to the progress of steam navigation.

GRAND PRIZE OF THE MATHEMATICAL SCIENCES: For any important advance in the theory of the application of electricity to the transmission of force.

VAILLANT: Fresh researches on fossils made in any region which for the last quarter of a century has been little explored from the palæontological standpoint.

DESMAZIERES: For the most useful work on the cryptogamous plants.

GRAND PRIZE OF THE PHYSICAL SCIENCES: On the mode of distribution of marine animals along the French seaboard.

1885

DELMONT: To engineers, for the best work connected with the Department of Public Works (Roads and Bridges).

FOURNEYRON: Theoretical and practical study on hydraulic accumulators and their applications.

DAMOISEAU: Review of the theory of Jupiter's satellites.

GRAND PRIZE OF THE MATHEMATICAL SCIENCES: Study of the elasticity of one or several crystallised bodies from the experimental and theoretical standpoints.

BORDIN: Researches on the origin of atmospheric electricity, and on the causes of the great development of electric phenomena in thunderstorms.

L. LACAZE: For the best treatise on physics, chemistry, and physiology.

DELESSE: For a work on geological sciences, or, failing this, on mineralogical sciences.

MONTAGNE: For important works on the anatomy, physiology, development, or description of the lower cryptogamous plants.

GRAND PRIZE OF THE PHYSICAL SCIENCES: Study of the intimate structure of the tactile organs in one of the chief natural groups of Invertebrates.

BORDIN: Comparative study of the freshwater fauna of Africa, Southern Asia, Australia, and the Pacific Islands.

GAY: Measure of the intensity of weight by the pendulum.

CUVIER: For the most important treatise either on the animal kingdom or on geology.

PETIT D'ORMOY: Pure or applied mathematical sciences and the natural sciences.

1886

DE LA FONS MELICOCQ: For the best treatise on the flora of North France.

1887

CHAUSSIER: For important works on legal and practical medicine.

1893

MOROGUES: For the most useful work in stimulating the progress of agriculture in France.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, April 24.—"On the Relation between the Electrical Qualities and the Chemical Composition of Glass and Allied Substances." Part I. By Thomas Gray, B.Sc., F.R.S.E., and Andrew Gray, M.A., F.R.S.E., Assistant to the Professor of Natural Philosophy in the University of Glasgow, and J. J. Dobbie, M.A., D.Sc. (Edin.), Assistant to the Professor of Chemistry in the University of Glasgow. Communicated by Prof. Sir William Thomson, F.R.S.

This paper describes some further experiments as to the relation between the chemical composition of glass and its quality of resistance to electrical conduction through its substance.

The experiments were made on specimens of flint glass of different densities, made by different makers, and varying considerably in chemical composition. The method of experimenting was that described in Mr. T. Gray's paper on the same subject (*Proc. R. S.*, No. 222, 1882). Each of the specimens, which were in the form of globular flasks of nearly three inches in diameter, was filled up to the bottom of the neck with mercury, and immersed to the same level in mercury contained in an outer vessel. A wire dipping in the mercury within the flask was connected, without touching the table or any of the supports, to one terminal of a sensitive galvanometer of high resistance, and the circuit completed, through a battery of about 120 Daniell's cells, from the other terminal to the mercury in the outer vessel. (The galvanometer was well insulated, and was the high resistance astatic instrument described in *Proc. R. S.*, February 14, 1884.) The outer vessel, containing the flask, was immersed in a sand-bath which was heated by a Bunsen burner to temperatures above 100° C., and readings of the galvanometer taken at different temperatures, with precautions to insure that there was no error due to leakage. By means of a suitable reversing key in the circuit, the direction of the electrification, which lasted in each case about three minutes, was reversed between each pair of readings. From the observed deflections, and the constants of the galvanometer and battery, which were frequently determined, the resistance of the flask at each temperature could be calculated.

The results of the electrical experiments and of complete analyses of the specimens of glass are detailed in the paper, and show that the specific resistance of glass of the kinds experimented on increases with the percentage of lead contained in the glass and also with the density; and that further, as had been previously found by different experimenters, the resistance diminishes as the percentage of alkali present in the glass increases. The best specimen experimented on contained over 40 per cent. of oxide of lead, and had a density of 3·141, and a specific resistance at 130° C. of 8400×10^{10} ohms between two opposite faces of a centimetre cube. From this result, as it has been found by these and former experiments that the resistance is halved, over a considerable range of temperature, by an increase of temperature of about $8\frac{1}{2}$° C., the approximate resistance of the glass at other temperatures may be found.

The paper concludes with a short statement of further work which the authors have in hand with respect to the electrical qualities of glass and minerals.

Linnean Society, May 1.—Prof. P. Martin Duncan, F.R.S., vice-president, in the chair.—Messrs. Wm. Dennison-Roebuck and F. Newton-Williams were elected Ordinary Fellows, and Profs. E. Haeckel of Jena, A. Kowalevsky of Odessa, and S. Schwendener of Berlin, Foreign Members of the Society. —Mr. S. O. Ridley exhibited and made remarks on a series of drawings of sponges (and their spiculæ) and of Actiniæ drawn from the living objects, as obtained in Ceylon and forwarded by Dr. W. C. Ondaatje, F.L.S.—Prof. Jeffreys Bell also drew attention to sketches of living Ceylonese Comatulids sent by Dr. Ondaatje as an earnest of progress in researches on the marine fauna of that coast.—Mr. T. Christy showed the leaf of a China grass rich in fibre useful for textile purposes.—Mr. R. Bowdler Sharpe read a paper on a collection of birds from

the Bahr-el-Ghazal province and the Nyam-Nyam country in Equatorial Africa. The collection had been made by Herr Bohndorff, who had spent several years in travelling over the region in question, and who brought a native Nyam-Nyam with him to the Society's meeting. Mr. Sharpe gave descriptions of and remarks on new species and on little-known birds, some twenty-seven in all, these being of considerable significance in relation to their faunal distribution. He pointed out that Herr Bohndorff had apparently crossed the boundary line of two faunas, for most of the Nyam-Nyam birds assimilated to those known from the Gaboon and Congo territory; whereas those obtained in the Bahr-el-Ghazal and Nilotic region were allied to the avifauna of North-East Africa and partly of the Senegambian area. Among new species cited are *Crateropus bohndorffi*, *Sigmodus griseimentalis*, *Mesopicus strictothorax*, *Centhinochares intermedius*, *Pionias bohndorffi*, and others of equal interest.—Mr. R. A. Rolfe thereafter gave a communication on the flora of the Philippine Islands and its probable derivation. According to recent computation the phænogamic vegetation of the Philippines consists of 3564 species belonging to 1002 genera. Of 165 dicotyledonous orders 119 are represented, and of monocotyledons 25 out of 35; while the three gymnospermeæ, though nominally there, are poor in number. The proportion of vascular cryptogams to phanerogams is nearly one-eighth, chiefly ferns. Of these 52 species are not known elsewhere, a fact stamping individuality on the flora. The endemic phænogamic vegetation consists of 917 species, or a proportion of over one-fourth endemic, the dicotyledons showing one-third, the monocotyledons about one-tenth. The striking feature of the flora is the large number of endemic species and the very small number of endemic genera. The flora approximates to that of the Malayan region, but very many typical Malayan genera—those even occurring on the neighbouring island of Borneo—are wanting in the Philippines. Taking into account the dominant Australian and Austro-Malayan features, along with numerous other data and reasoning, Mr. Rolfe infers that Mr. Wallace's idea of extinction of genera by submergence will not alone explain the present peculiarities of the vegetation. Mr. Rolfe looks upon the Philippines as truly insular in the essentials of their natural history, this not so much through their being an early separation from the Asiatic continent which has had a dip under the sea, as from their being largely of volcanic and geologically of somewhat recent origin.—Mr. Geo. Brook read a preliminary account of the development of the weaver fish (*Trachinus vipera*). In this he mentioned that the eggs had been laid in his aquarium at Huddersfield, the fish themselves having been kept alive therein over two years. He drew attention to the fact of there being a vitelline membrane present in the eggs of this fish, as well as in those of the herring; in contradistinction therefore to what is stated to be the case in osseous fishes generally. He also particularly referred to the persistent nature of the segmentation cavity, which is pushed round the yolk-sac concurrent with the development of the embryo from the blastoderm; and that it does not entirely disappear until the yolk is absorbed. The circulatory system, according to Mr. Brook's researches, is very late in developing, no blood-vessels appearing until several days after hatching. In illustration of his paper he exhibited under the microscope preparations showing the segmentation stage, the embryonic shield, and commencement of keel, the early embryo third day before closure of the blastopore, and fourth-day blastopore and Kupffer's vesicle, also at the eighth day, and the newly-hatched embryo.—Dr. J. Millar and Mr. J. Jenner Weir were elected auditors for the Fellows, and Mr. T. Christy and Mr. H. T. Stainton for the Council.

Mathematical Society, May 8.—Prof. Henrici, F.R.S., president, in the chair.—Mr. J. Brill was elected a member, and Prof. Luigi Cremona, of Rome, Foreign Member, was admitted into the Society.—Prof. Cremona communicated, in French, a paper entitled "Sopra una trasformazione birazionale, del sesto grado, dello spazio a tre dimensioni, la cui inversa è del quinto grado." Dr. Hirst, F.R.S., welcoming the author, spoke in commendatory terms of the value of the communication.—The following papers were also laid before the Society:—Motion of a network of particles with some analogies to conjugate functions, by E. J. Routh, F.R.S.—On a subsidiary elliptic function, by J. Griffiths.—On the homogeneous equation of a plane section of a geometrical surface, by J. J. Walker, F.R.S.—On the "symmedian-point" axis of a system of triangles, and on another line which is connected with a plane triangle, by R. Tucker.

Chemical Society, May 1.—Dr. W. H. Perkin, F.R.S., president, in the chair.—The following papers were read:—On benzoylacetic acid and some of its derivatives (part i.), by W. H. Perkin, jun. For various reasons the author determined to examine carefully benzoylacetic ether with special reference to reactions in which the ketone group takes part. Full details of the preparation of this body, which boils at 265° to 270°, and gives a violet coloration with ferric chloride, are contained in the paper. When boiled with dilute sulphuric acid, it splits up into acetophenone, alcohol, and carbonic anhydride. The barium, silver, copper, and lead salts were prepared. The paper contains an account of the preparation and properties of the following bodies: Benzoylacetic acid, ethylbenzoylacetic acid, diethylbenzoylacetic acid, allylbenzoylacetic acid, the corresponding ethers and their decomposition products, and an investigation of the action of bromine on allylacetophenone.—The composition of coal and cannel gas in relation to their illuminating power, by P. F. Frankland. In this paper the author gives the results of a detailed examination of the gas supplied to some of the more important towns of the United Kingdom. The constituents which have been determined are the hydrocarbons absorbed by fuming sulphuric acid, carbonic anhydride, oxygen, nitrogen, hydrogen, carbonic oxide, and marsh gas. The results are compared with previous analyses in 1851 and 1876.—On selenium sulphoxide; on the reaction between hydrogen chloride and selenium sulphoxide; on selenium selenochloride, by E. Divers and Masachika Shimosé.—On a new form of pyrometer, by T. Carnelly and T. Burton. This consists essentially of a coil of copper tube, which is placed in the furnace, oven, &c.; through this coil flows a constant current of water; the temperature of the oven is estimated by the difference between the temperature of the water as it flows into and issues from the coil.—On fluorene, by W. R. Hodgkinson. During the fractional distillation of fluorene the formation of an orange-red substance was noticed; this seemed likely to be an oxidation product, and in the present paper the author gives an account of his attempt to isolate this body, which is rendered extremely difficult, as the substance decomposes when distilled in a vacuum, and is equally soluble with the hydrocarbons which accompany it.

Institution of Civil Engineers, April 22.—Sir J. W. Bazalgette, president, in the chair.—The paper read was on the comparative merits of vertical and horizontal engines, and on rotative beam-engines for pumping, by Mr. Wm. E. Rich.

EDINBURGH

Royal Physical Society, April 23.—Dr. Ramsay H. Traquair, F.R.S., president, in the chair.—Mr. Hugh Miller, of H.M. Geological Survey, read a paper on boulder glaciation and striated pavements, an abstract of which was given in these columns on May 1 (p. 23).—The President gave an outline of a paper by Mr. J. T. Richards, on Scottish fossil cycadaceous leaves contained in the Hugh Miller collection.—Mr. J. R. Henderson exhibited various mollusks and zoophytes from the Firth of Forth.—Mr. Henry Gunn, A.R.S.M., contributed a paper on the silver districts of Colorado (Leadville and San Juan). In the first portion of the paper, which dealt with the Leadville deposits, the author pointed out that within a limited thickness of from 700 to 1000 feet, typical representatives of Laurentian, Cambrian, Silurian, and Carboniferous rocks were to be found, and also indicated the influence which intrusive rocks had in the economic geology of the district, inasmuch as all the deposits occurred at the contact of the quartz porphyry with the limestones. Specimens illustrative of the ores mined in the district were exhibited, also some possessing unusual associations of mineral, a specimen showing granules of free gold in hard carbonate of lead attracting much attention from the fact that it was the only specimen ever discovered in the district exhibiting this association. Mr. Gunn exhibited specimens of tellurium ores of remarkable beauty, and a sample of zinc blende mined in large quantity in Pitkin County, which, contrary to the opinion generally held by miners, contained large quantities of silver. The second portion of the paper dealt with the San Juan district, and after indicating the peculiar disadvantages under which this district laboured for the first few years of its existence, proceeded to describe the geology of the district, which, he states, to be Trachyte overlying rocks of Carboniferous and Devonian age. The mineral is found in true fissure veins of great width, chiefly composed of quartzose matter, but usually carrying one or more gray streaks from two to six feet wide, composed of

galena, fahlertz, and sulphurets of silver and gold. Some of the mines produce beautiful filaments of native silver, and one of the specimens showed a very unusual association, viz. fine filaments of silver on gray copper.—The Secretary (Robert Gray, V.P.R.S.E.) exhibited a specimen of the Calandra lark (*Alauda calandra*) from the neighbourhood of Madrid, showing a peculiar malformation of both mandibles, which seemed to render it impossible for the bird to pick up its food. The specimen had been sent to him by Dr. A. C. Stark, and is to be deposited in the Edinburgh Museum of Science and Art.—Mr. Gray also reported the occurrence of at least three instances of the stock dove (*Columba œnas*) in Roxburghshire, and made some remarks on the distribution of the species in the border counties. This bird has now been found to be a regular visitant to the counties of Berwick, Dumfries, and Roxburgh, in all of which it breeds.—Mr. Harvie-Brown, F.R.S.E., F.Z.S., exhibited, with remarks, a specimen of the black redstart (*Ruticilla titys*, Scop.), taken last month on the Pentland Skerries, Pentland Firth. The specimen was a male adult, and is said to be the fifth of the species recorded in Scotland.

Mathematical Society, May 9.—Dr. Thomas Muir, F.R.S.E., president, in the chair.—Prof. Crum Brown delivered an address, interesting alike to mathematicians and to chemists, on the hypothesis of Le Bel and Van't Hoff.—Dr. Muir gave a preliminary account of a treatise on Determinants, published in 1825, and unknown to all writers on the history of the subject.

DUBLIN

Royal Society, April 21.—Section of Physical and Experimental Science.—Arthur Hill Curtis, LL.D., in the chair.—Notes from the Physical Laboratory of the Royal College of Science, by Prof. W. F. Barrett.—On the local heliostat, by G. Johnstone Stoney, D.Sc., F.R.S. This instrument was designed by the author many years ago, and made for him most satisfactorily by Messrs. Spencer and Sons, opticians, of Dublin, who have since constructed several reproductions of it for physicists at home and abroad, at the suggestion of one of whom it is now described. The instrument is in some degree a modification of Gambey's heliostat, but it differs from that apparatus by being simpler in its details, steadier, easier to use, and cheaper. These advantages are gained by sacrificing the generality of Gambey's instrument and providing only for stations within a limited range of latitude, usually about 10°, which, for example, enables one instrument to be used anywhere within the British Islands. Hence it has been called the local heliostat. The adjustment for latitude is of the simplest kind. After it is made, the instrument is to be levelled, and an arrangement based on the principle of the sun-dial enables it in about half a minute to be placed in the meridian. A polar axis is driven by a common clock at the rate of one revolution in twenty-four hours. To the upper end of this axis an arm is jointed, which, by a simple contrivance, can be pointed towards the sun, and which the clockwork, while in action, will then cause to follow that luminary. This arm trammels the mirror in the same way as in Gambey's instrument; and the reflected ray continues in the direction of a bar which can be placed in any azimuth and can be inclined up or down within reasonable limits. The direction of this bar, and with it of the reflected ray, can be readjusted, if necessary, in one or two seconds without disturbing the rest of the apparatus. The local heliostat has hitherto been made with mirrors about six inches by three for use in physical laboratories, but the design has been rendered so simple that it could be made at small cost with a mirror as large as a toilet glass, and driven by a cheap common clock. This would furnish an instrument which might be employed in physiological experiments on plants, in photography, and for any other purposes in which a large sunbeam in a fixed direction would be useful.—Dr. C. E. Fitzgerald exhibited Mr. P. Smith's model illustrating the conjugate movement of the eyes.

Section of Natural Science.—V. Ball, M.A., F.R.S., in the chair.—The following papers by D. Sharp, M.B., were communicated by Prof. W. R. McNab, M.D.:—(1) Descriptions of new genera and species of Hawaiian Coleoptera; (2) Catalogue of Hawaiian Coleoptera, with localities, distribution, and habits; (3) Topographical table of Hawaiian Coleoptera, with summaries, generalisations, and comments.—Prof. A. C. Haddon, B.A., F.Z.S., on the generative and urinary ducts in Chitons. The author discussed the various views as to the nature of the urinary ducts in Chitons. His own investigations supported Sedgwick's account as opposed to Haller's, and an oviduct was proved to exist in *Ch.* (*Trachydermon*) *ruber*, Linn.—Notes on some of the Irish crystalline iron ores, by G. H. Kinahan, M.R.I.A.—Additional notes on the phenomena attending the eruption of Krakatoa, by J. Joly, B.E. These included an account, received from Capt. Thomson of the *Medea*, relating to phenomena observed on August 22 to 24, while the *Medea* was in the Sunda Straits. These embraced electrical effects, showers of sand and gravel, &c. At 2 p.m. on the 26th Capt. Thomson heard the first explosion; others succeeded every ten minutes. This geyser-like regularity was substantiated by all accounts received by the author. A column of dust arose to the westward immediately after the first explosion. Two observations enabled the height of this column to be computed. One gave seventeen, another twenty-one miles. The last included some doubtful factors, the observation being made three hours after first explosion. Further examination of the dust and pumice revealed hematite in thin blood-red flakes. The feldspars seemed divisible into two groups depending on optical and structural differences. Pyrites occurred not alone as an inclusion in the feldspars but also in the hypersthene. Optical tests rendered highly probable the presence of a triclinic pyroxene.

MANCHESTER

Literary and Philosophical Society, March 4.—H. E. Roscoe, F.R.S., president, in the chair.—A paper was read on the production and purification of gaseous fuel for industrial purposes, with the results of several large applications of a system, by W. S. Sutherland.

March 18.—H. E. Roscoe, F.R.S., president, in the chair.—Notes on the meteorology and hydrology of the Suez Canal, by Dr. W. G. Black, F.R.Met.S.

VIENNA

Imperial Academy of Sciences, April 24.—F. von Hochstetter, fifth communication to the seventh report of the Prehistoric Commission; on the tumuli at Froeg, near Rosegg (Carinthia).—K. Zulkorosky, on the aromatic acids as dye-forming matters.—W. Stephanie, on rotation of the moon.—T. Unterweger, on the aurora borealis.—T. Habermann and M. Hœnig, on the action of cuprum hydroxide on some sugars.—F. Berger, on the preparation of phenylcyanide.—E. Spiegler, contribution to a knowledge of the euxanthone group.—Contribution to a knowledge of diphenylglucetoxin, by the same.—T. von Hepperger, on the position and figure of isochrones in comets' tails.

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